August 2019

THREE ESSAYS ON PROPERTY TAX ADMINISTRATION

Yusun Kim
Syracuse University

Abstract

This dissertation is comprised of three essays on real property tax administration and related state-local fiscal relations. All three essays exploit variation in state policies as natural experiments to study the various features of the property tax system.

The first essay examines how county governments respond to a state policy that reduced counties' share of state Medicaid costs, in a state where counties are mandated to financially contribute to the state program. The key motivation of this study is to understand the consequences of a change in the way a large public insurance program is co-financed by different levels of governments. This paper intends to fill the gap in the literature by providing empirical evidence on how local governments respond to a sudden *decrease* instead of an *increase* in the outlay of a large mandatory spending category. Utilizing the plausibly exogenous decline in county Medicaid spending in New York between 2005 and 2006, I estimate its impact on various fiscal outcomes including non-Medicaid budget and effective property tax rate, by using difference-in-differences and event study estimators. I find no income effect on other spending but evidence of significant property tax relief among affected counties in New York. The findings suggests that reallocation of limited state and local resources or fiscal responsibility through changes in cost sharing may have spill-over effects on local fiscal decisions.

The other two essays examine the effects of two key institutional elements of the real property tax on the cost and outcome of property assessment – the size of tax assessment jurisdictions and the length of property assessment cycles. The second essay estimates the returns to scale in property assessment. This essay focuses on tax assessing jurisdictions in New York that unified assessing functions with neighbors, forming a coordinated unit in response to state aid. Using a cost function framework, this essay tests whether merging assessing functions among assessing jurisdictions leads to cost savings. We employ multiple instruments to address the potential selection bias of each jurisdiction's decision to form a coordinated unit. The instruments are based on spatial intersection across jurisdiction boundaries and the history of inter-municipal cooperation among neighboring jurisdictions.

The third essay examines the effect of regular, short cycles on assessment performance, by using two separate case studies of assessing jurisdictions in a representative strong "dillon state", Virginia and a strong "home rule" state, New York. Outdated property assessment is widely believed to undermine equity and cost efficiency in administering the property tax. This essay first tests the effect of frequent mass appraisal on assessment uniformity, using exogenous variations in the timing of reassessment across assessing jurisdictions in Virginia. Then it examines whether more frequent (consecutive annual reassessment, in particular) leads to improvement in horizontal equity among assessing jurisdictions in New York, employing two stage-least square and semi parametric event study estimators.

THREE ESSAYS ON PROPERTY TAX ADMINISTRATION

by

Yusun Kim

B.A., Korea University, 2011

M.A., Seoul National University, 2013

Dissertation

Submitted in partial fulfillment of the requirements for the degree of

Doctor of Philosophy in Public Administration.

Syracuse University

June 2019

ACKNOWLEDGEMENTS

First of all, I would like to express my earnest gratitude to my academic advisor, Professor Yilin Hou who has trained and supported me throughout my academic journey over the past five years with great patience. I am deeply in debt for his trust, encouragement, and guidance. I am also sincerely grateful for the valuable comments and career advice I received from my co-authors, John Yinger and Sarah Hamersma. I genuinely appreciate the encouragement and detailed feedback on my dissertation provided by Michael Wasylenko, Michah Rothbart.

I offer special appreciation to my parents, Hyungsun Kim and Soolee Kim for their unconditional love, understanding and sacrifice. I also owe thanks to my parents in law for their prayers and encouragement. Finally, I am truly thankful for my husband – Keonho Kil for being by my side. I would not have been able to make it this far without him.

Syracuse, New York

May 2019

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Chapter 1.

How Reduction in Mandated Medicaid Spending Affects Local Fiscal Behaviors: Evidence from New York State

Abstract

This paper aims to advance empirical understanding about local fiscal responses to an intergovernmental fiscal policy that changes the way two levels of governments share the costs of a large public social insurance program. New York State passed a legislation in 2005 to cap the growth of county-level Medicaid spending, which abruptly decreased county Medicaid outlay in relative and absolute terms. This paper exploits a discontinuity in county Medicaid outlay to analyze the impact of the relief mandate on county budgets and property tax levies. This study helps to fill a gap in the literature on how local governments respond to a sudden *decrease* instead of an *increase* in the outlay of a large mandatory spending category, which could potentially lead to property tax relief. I find compositional change but no income effect on non-Medicaid spending. The results show that effective property tax rate is lower by 6.6 to 8.1% on average among affected New York counties after enactment of the cap policy, relative to comparison counties. This essay contributes to the empirical literature on the various indirect channels for property tax relief.

JEL codes: H5, H7

Keywords: Intergovernmental fiscal relation, Local fiscal behavior, Medicaid, Property tax relief, State mandate

1. Introduction

What are the consequences of a higher-level government's policy decision to abate the lower level government's share of financing a large public insurance program? One of the major concerns of intergovernmental relations in a federal system is understanding how certain changes in the way sub-national governments at different levels share costs of providing public services may affect their fiscal behaviors. This paper specifically studies the fiscal responses of local governments when the New York State government in the United States reduced the local burden to finance Medicaid, a public health insurance program for the poor, in 2005. The primary rationales of the 2005 Medicaid cap legislation were to relieve county governments from the fiscal stress to deal with strained budgets and to prevent them from cutting back local spending in response to growing Medicaid costs. There are mainly two reasons why this policy needs to be examined.

The public finance literature is relatively long on the consequences of an increase in one spending category at the expense of other categories. However, little is known about how local governments respond when there is a sudden *decrease* in local spending in a large spending category, as opposed to an *increase*, which is what happened when the New York (NY) state enacted a local Medicaid mandate relief in 2005. As fiscal constraints start to bite harder, the pressure to pay for expensive entitlement programs such as Medicaid grow for governments at all levels, but particularly for local governments that have the most limited tax base. Like other entitlement programs, Medicaid is continuously expanding and in fact the fastest growing component of state and local government budget. Previous research debates the extent to which State Medicaid crowds out other programs such as K-12 public education (Berry and Lowery

1990; Baicker 2005). Although downsizing an entitlement program is generally considered as politically infeasible, it is still important to understand the consequences of such policy change in an era of retrenchment.

Second, Medicaid is largely funded by the federal and state governments and administered by the states, but in some states, local governments play a considerable role. More than half of the state governments in the U.S. require localities to financially contribute to the state Medicaid program. Among those states, New York requires the highest level of local contribution from its localities. NY is also an interesting case to study since it has a generous Medicaid program and relatively high local property tax rate when compared to its neighboring states[1]. Thus, how to lower local tax burden while maintaining an expensive Medicaid program in a financially sustainable way is an imminent policy concern that requires scrutiny.

The local fiscal responses to this 2005 mandate relief policy will be analyzed in two steps: The primary step to understanding the potential impact on property tax relief is to see how spending on other categories changed immediately after the cap policy was enacted. The first research question raised in this paper is the following: (1) What happens when there is an exogenous *decrease* instead of an *increase* in Medicaid spending and what bearings it may have on the provision of other public services. By studying this reverse case of budgetary crowd out, we can check the asymmetry in the budgetary tradeoff, which has been neglected in the existing literature. A government's decision to increase spending in one category may simply lead to compositional changes at the expense of other public programs, while not affecting the total budget to avoid raising the tax rate. However, part of the existing scholarship on the flypaper effect suggest that intergovernmental grants are likely to encourage aid recipients to expand their

[1] In 2017, New York State had the highest Medicaid liability, which amounted to 27 billion US dollars (Office of the New York State Comptroller 2018).

budgets which often creates the need to increase own source revenue. If we can treat state-mandated reduction in mandatory spending as a variant of a state grant, it is meaningful to analyze how the NY counties respond to the mandate relief policy. In terms of budgetary response, they can either simply redirect their limited resources to other programs within their existing budget constraint, or they can increase their total outlay; a phenomenon which can be akin to flypaper effect. Depending on their budgetary decisions, they can either choose to provide tax relief to local taxpayers or increase tax levy to finance their expanded budget. By construction, property tax may play a balancing role in the local budgeting process. This leads to the second question raised in this research: (2) Did the 2005 Medicaid mandate relief policy lower county-level property tax burden in New York?

This paper first analyzes whether a sudden decrease in one spending category led to changes in expenditure on other programs using event study framework. Then this study checks whether the policy generated a reduction in effective property tax rate (ETR) at the county level. Using variations across counties within NY State and then comparing NY counties against counties from comparable states with a difference-in-differences estimator, this paper reports evidence of local property tax relief caused by Medicaid mandate relief. This study suggests that certain changes in the way state and local government reallocate their resources to fund an expensive public program may lead to property tax relief.

2. Literature Review

The first step is to understand how the governments affected by the mandate relief change their budgetary decisions when there is a sudden decrease in one large expenditure item. The second step is to assess the impact on property tax relief.

2.1 Budgetary Trade-off

In regard to the first question, early scholarship on public budgeting separated politics from budgets and demonstrated that each expenditure item is independent of each other, as they should be (Wildavsky 1964; Dye 1966). Since the 1980s, political scientists started to counter this view, underlining that budget decisions intrinsically reflect political competition and policy priorities (Natchez and Bupp 1973; Garand 1985; Schick 1988; Alt and Lowery 2000; Nicholson-Crotty et al. 2006; Jacoby and Schneider 2001). Several scholars asked in what spending categories or policy area the relation between politics and budgeting would be the strongest (Garand and Hendrick, 1991; Jacoby and Schneider, 2001). Schick (2000), in his depiction of the American federal budget, argues that the American federal budget is set up in a way that entitlement and defense obligations crowd out discretionary spending in other categories.

A number of studies analyze the extent to which an increase in either health or education spending crowds out the other since these two are the largest spending categories at the state and local level (Domke et al. 1983). In the Medicaid literature, Baicker (2005) shows that additional mandated increase in Medicaid expenditure crowds out other public welfare spending. Several other commentators, on the contrary, have suggested that health spending is relatively immune to budget-cuts (Berry and Lowery 1990; Coughlin et al. 1994)[2].

Although this paper addresses a similar question of crowd-out or spillover effect on other spending, it mainly analyzes the behavioral responses of a slightly different event. The question is what happens when there is an exogenous *decrease* instead of an *increase* in Medicaid

[2] Coughlin and his colleagues (1994) find that state governments are reluctant to enact large-scale cuts in Medicaid spending and instead seek for alternative budgetary actions including incremental program cutbacks; constraining other spending; shifting fiscal burden to the federal government and ultimately raising state tax rates.

spending and what bearings it may have on other expenditure. While the general concern is how increasing burden of entitlement programs impose fiscal stress on governments and diminishes their discretionary budget, we know little of what happens when there is a sudden *decrease* in obligatory spending.

In which direction other discretionary budgets might change, as a response to a reduction in a major obligatory spending category is rather ambiguous. There are four possible scenarios for county governments' budgetary decisions. In the first scenario, there could be no change in other spending except for Medicaid and thus, lowering total spending. Alternatively, there could be either no change or increase in total spending, depending on how much the recipient governments augment other discretionary budgets. In the fourth scenario, county governments may cut other discretionary budgets as well, should they feel the need to undertake fiscal consolidation[3]. In this paper, we would expect to find evidence in support of the second or third scenario. In other words, it is likely that the county governments may substitute away from Medicaid and simply reallocate their limited resources to other direct services, which would be a mere compositional change without dramatically changing the total current services budget. However, depending on how much the counties decide to utilize the fiscal space generated by the cap policy, there could be a simple substitution effect.

This paper examines the impact of state mandate relief, which may or may not have similar effect as a direct grant. The 2005 Medicaid cap policy reduced the amount of Medicaid cost each county government was mandated to pay back to the state government. This creates fiscal space among the affected county governments, which can lead to responses analogous to

[3] Although this could be accompanied by income effect in theory, the amount that counties spend from their general expenditure is capped by the mandate relief, thus the if the recipient government decide to spend more on anything it would be on other programs.

those from an increase in grant receipt. In response, the counties that benefit from this policy may either choose to increase their total operational budget and accordingly raise their own source revenue[4]. In the absence of such response, on the other hand, there might be no significant increase in the total outlay. If there is also no substitution effect between Medicaid and other public spending, total spending could even decrease which may provide an opportunity for reducing own-source revenue and providing property tax relief, in particular.

2.2 Direct and Indirect Property Tax Relief

In the public finance literature, there are two types of state-initiated property tax relief programs. A direct type explicitly targets lowering property tax burden as the primary policy goal, while an indirect type has unintended or indirect spillover effects on local property tax (Duncombe and Yinger 2001). Among the latter, the most extensively studied cases are state aids provided to local governments. The most traditional equivalence literature suggested that some state intergovernmental aids have a similar impact on lowering the tax burden of median taxpayers as state-funded property tax cuts (Bradford and Oates 1971). Duncombe and Yinger (2000) find that state aid leads to property tax cuts, ultimately relieving tax burden among local voters in the State of New York. Nguyen-Hoang and Hou (2013) also report that grant receiving municipalities in another American state, Massachusetts, did not provide property tax relief when there was a significant cut in state aid between 2008 and 2009. The authors find that the municipalities opted for financing capital projects or saving for future liabilities in response to grant cuts.

[4] This would be unlikely when there is sufficient interstate competition that works as a constraint on public spending, assuming Tiebout's logic of private firms and individuals "voting with their feet." *Ceteris paribus*, firms would avoid moving to or move from areas with higher tax rates to fund generous redistributive social programs. Therefore sub-national governments will refrain from becoming welfare magnets and compete with each other to keep their level of social benefits low[4] (Besley and Case 1995; Thompson 2012). Citizens tend to assess whether the local governments are wasting their tax money which constrains the size of government spending and restricts the growth of a revenue-maximizing Leviathan.

Another case of an intergovernmental fiscal policy that may indirectly affect local property tax is a takeover of what previously used to be local spending responsibilities by the higher level government. One example is a study by Olmsted and his colleagues (1993) that examines state aid in the form of a retirement of debt issues that were expected to affect fiscal behaviors of local school districts in Missouri State. They found that debt retirement that allowed school districts to no longer pay the state government with regular interest payments, did not lead to tax cuts but increased operating budgets. This paper intends to fill the gap in the literature by analyzing another variant case of an intergovernmental policy that may have an indirect effect on fiscal behaviors among lower-level governments, in a federal system.

3. Medicaid Local Mandate in New York State

The U.S. federal government allows each state government to run the state Medicaid program, a public health insurance program for the poor, at their discretion. As long as the state governments abide by the federal requirements, the federal government reimburses half of the total Medicaid costs to each state. 22 states cover the other half of the total costs from own source revenue; New York State is one among 19 states that require the local governments to pay a certain share of the non-federal costs[5]. New York State included a local share mandate that required county governments to share the burden in 1966 when the state government first enacted the Medicaid program. The local share requirement was initially 50 percent of non-federal Medicaid costs and 25 percent of the total cost. While New York state government has a long history of sharing costs to provide public services with its localities, an across-the-board

[5] There are 28 states in total where local governments share the burden of non-federal Medicaid costs, with or without a state mandate of local requirement.

reallocation of funding responsibility was a more recent event[6]. Facing fiscal burden induced by increasing Medicaid costs and limited tax base, counties began to complain against the even division of responsibility for financing Medicaid[7]. Such movement in the political arena was soon translated into changes in the state government's decision over funding Medicaid.

In response to the rising complaints from local governments, the NY state government established a cap on counties' share of total Medicaid expenditure that applies universally to all counties in the state fiscal year 2005. First, the state fully took over local contribution to Family Health Plus program, which is a Medicaid expansion program, effectively completed by 2006. More importantly, the state capped county share of Medicaid spending at each county's 2005 baseline costs estimated by the State, which would be adjusted by a certain annual growth rate set by the statute (Part C of Chapter 58 of the Laws of 2005)[8]. The statute also set the growth rate to be adjusted by uncompounded trend factors: The rates were 3.5 percent in 2006, 3.25 percent in 2007 and 3 percent each year from 2008 onwards until 2013[9]. All exceeding costs that go above the growth rate were to be covered by the state government. The state also relieved the counties of existing liabilities for Medicaid in 2005 under the accrual accounting system. In the original system, counties had to set up reserve funds to pay for Medicaid services expected to be provided in a given year but not paid until the next calendar year. From 2005, the state converted the accounting methodology to a cash-based accounting system, where the counties started to budget their Medicaid expenses in the year of payment instead of the year of service provision.

[6] State policymakers gradually corrected the local share arrangement during the 1980s. Yet, there was no fundamental policy to adjust the local share across all services until the mid-2000 (Bachrach and Burghardt 2006).

[7] Especially, since the enactment of the Family Health Plus, which was a part of the Health Care Reform Act (HCRA) in 2000, local fiscal burden rose considerably.

[8] The 2005 baseline costs are estimated primarily using 2004 data using individualized worksheets provided by the State to each county (Birnbaum 2010). The capped amounts are expected to be paid by counties to the state government in weekly installments starting from calendar year 2006. In New York, each state fiscal year runs from April 1st until March 31st. The cap was effective from January 2006 calendar year, which is 2005 state fiscal year.

[9] The growth rate was capped at 2 percent for 2013-2014, according to the NY State annual reports.

This created one-time positive net savings to a majority of counties in 2005[10]. This savings is partially offset by elimination of accrued revenue receivables in 2005 which used to be typically paid by the state to counties at the end of each year for Medicaid recoveries and reimbursement for mentally disabled. The net effect of the expenditure and revenue accrual closeout process and cap of future growth rate led to positive savings for 45 counties between 2005 and 2006. However, there were twelve counties that did not experience a reduction in Medicaid expenditure after 2005. These counties may not have been affected by the cap policy, either because the growth in their Medicaid spending was less than the capped growth rate or experienced a greater loss in revenue accruals, because they used to spend more on the mentally disabled than other Medicaid programs where they could have benefited from expenditure accrual. Either way, the reason why the 12 counties did not benefit from the cap policy is exogenous to the counties' fiscal behaviors. I use the variations between 45 affected and 12 unaffected counties as part of the identification strategy in the within-NY analysis.

The direct intermediary effect of the statute on affected NY counties is a reduction in local Medicaid outlay, both in absolute and relative terms[11]. As shown in Figure 1 - Panel A and Panel B, introduction of the Medicaid spending cap immediately led to a sharp fall in counties' share of Medicaid spending in 2005[12]. In addition to a noticeable discontinuity in average county-level Medicaid spending in 2005, the slope also changes after the cap is implemented, as illustrated in Panels A and B. The increasing trend in county portion of Medicaid cost is reversed

[10] Also, from 2008, counties have the options to either continue paying the capped amount that grows at maximum three percent or to get rid of the burden to fund Medicaid altogether while swapping a portion of their local sales tax revenue with the State. As of 2011, only Monroe County elected this sales tax swap option.

[11] The highest drop in Medicaid spending in 2005 was found in the Counties of Delaware, Erie, and Essex. There also were several counties (Hamilton; Putnam; Tioga and Tompkins) that did not experience any fall in Medicaid spending in that year, mainly since their average Medicaid expenditure were below the baseline cap.

[12] The cost-sharing ratio varies by subprograms: The State and local governments each evenly divided the non-federal share for acute care services; while, the State covered 40 percent, and localities covering the remaining 10 percent for the non-federal cost of long-term care services.

after 2005 which coincides with the period when the NY state government enacted the Medicaid cap legislation. The empirical contribution of this paper is to understand the consequences of this particular local mandate relief, by focusing on the impacts of the abrupt decrease in local Medicaid spending.

After adoption of the cap policy, the price for Medicaid spending above the capped limit at the county level essentially became zero. In a typical case, one can expect county's maximizing behaviors of increasing Medicaid provision, that may offset the impact on other types of spending. However, there is no evidence of such price effect on Medicaid spending, due to the limited role of county governments in administering Medicaid. The generosity of Medicaid in NY is mostly (almost entirely) determined by the State and the counties do not play an important role that would affect the level of Medicaid outlay. The counties have some discretion in processing applications and monitoring enrollment and utilization. Perhaps they could become more lenient in admitting Medicaid applicants after the policy enactment. However, the extent to which this could translate into an increase in Medicaid outlay is very limited. Panel B in Figure 2 shows that the average count of Medicaid enrollment across all counties in New York is in fact declining between 2005 and 2008. In addition, the county governments may not have the strong incentives to expand Medicaid should they fear becoming welfare magnets, as suggested by inter-jurisdictional competition in the public finance literature. Although we find no increase in Medicaid spending, the combination of income and substitution effect may lead to changes in other spending, the direction of which is ambiguous.

4. Sample and Data

The unit of analysis in this study is county governments. The first part of the analysis exploits the heterogeneity across 57 upstate counties within the New York State, which will later become the treated group in the difference-in-differences analysis. The treated group excludes five counties in New York City, which are outliers in terms of the level of Medicaid spending and the role of county governments[13]. The final sample covers 10 years from 2001 to 2010: The sample years are limited to the period before 2011 when the state enacted a property tax cap on local governments, which may conflate the effects on property tax relief[14].

All revenue and expenditure data on New York counties are from the Open Book annually published by the New York Comptroller's Office. The state total Medicaid expenditure data are from the Annual State Expenditure Report from the National Association of State Budget Officers (NASBO). Local property tax rate data is collected from the New York State Office of Real Property Tax Services (ORPTS). The county-only-ETR is calculated by using real assessed value tax rates and multiplying this figure by local equalization rates, which is equivalent to assessment ratio calculated by the state[15]. Property tax levy and tax rate data are reported by individual local governments through schedules of real property taxes and assessments (MA-144) to the state, while local equalization rates are computed by the New York State Office of Real Property Tax Services[16]. Table 1 and 2 provides summary statistics of the observable characteristics of counties within New York State.

[13] Monroe County, is the only county that chose the sales tax intercept option as an alternative option to abiding by the Medicaid spending cap. This option was provided since 2008 and there is a three-year difference, between the treatment year. Although this is not expected to affect the short-term impact of treatment, it may affect the β_3 estimates. Monroe is dropped from the final sample; however, the reported estimates are not sensitive to the inclusion of Monroe County.

[14] Each year refers to county fiscal year, which is the calendar year. These years are the year when the county government establishes an assessment roll for levying property tax and not the tax levy year.

[15] The NY state has been using the median sales ratio since 1992 to calculate state equalization rate.

[16] Counties, towns, cities as well as school district in the state of New York collect property tax.

In the second analyses using difference in differences, a larger sample was incorporated. I use various subgroups of comparison units from a larger pool of 1,309 counties in 21 states that maintained local Medicaid requirement and 1,117 counties in 19 states that had no property tax levy or rate limitations. Property tax levy per capita from the Annual Survey of State and Local Government Finances is used as the alternative measure of the dependent variable. Annual housing price index provided by the Federal Housing Finance Agency is also used to control for the fluctuations in the housing market.

In the following analysis, I use ETR of counties from seven states outside of New York that were unaffected by the NY cap policy as the counterfactual. County-level effective property tax rate data was collected separately from each state's department of taxation or comptroller's office. Median assessment sales ratio data for each state was collected to calculate the county-only real effective property tax rate. The seven comparison states were selected based on the administrative role the county governments plays, such as levying the property tax as in New York[17]. We also exclude states that do not provide data on county-level property tax rate or have data that are incomparable to that of New York from the pool of potential control states. The seven comparative states included in the final sample are Maryland, New Jersey, Iowa, Virginia, Kentucky, Tennessee and Georgia[18]. In the final balanced panel sample for the second analysis, 584 counties are in the control group and 45 New York counties are in the treated group.

Data for covariates such as median household income, population and poverty rate are from the U.S. Census, while county unemployment rate data is from the Bureau of Labor Statistics. Also, one should note that each year (unless labeled as state fiscal year or calendar

[17]. For instance, Connecticut and Rhode Island are excluded since these states do not have county-equivalent jurisdictions.

[18] Some states provided the countywide total that includes overlapping municipal areas while others provided county-only nominal property tax levy. The seven states included here report the real property tax rate for unincorporated and non-municipal county-only areas excluding school or special districts.

year) indicates the end of a fiscal year the county governments were surveyed.[19] One of the concerns about using counties from other states as the comparison group would be the heterogeneity in the pressures of growing Medicaid costs and generosity. While Medicaid expenditure for other counties outside NY is not employed in the main analysis, it is worth mentioning that the trend in total state Medicaid spending was stable during the post period. Parental Medicaid income threshold which is used as a key eligibility criterion for Medicaid is also included to account for the heterogeneity in state's generosity for providing Medicaid (Hamersma & Kim, 2013)[20]. Tables 2 provides a summary statistics of the variables included in the final sample using variation across states.

5. Empirical Identification Strategy

5.1 Spillover Effects on Other Spending: Empirical Models

This study takes advantage of the fact that the sudden drop in county Medicaid spending between 2005 and 2006 in NY State is exogenous to the fiscal decisions made by the county governments. County residents and legislators had long been filing complaints about increasing Medicaid costs crowding out limited resources for other public goods and services at the county level, such as community colleagues and highways. Therefore it is worthwhile to investigate whether the state policy of capping local Medicaid spending can relieve local fiscal burden by moderating cutbacks in public spending or lowering the need to increase the tax rate.

Ideally, the current services budget should be balanced, i.e. total outlay should match with total revenue in the general fund account. Next year's total expenses are expected to be covered by

[19] For instance, the 2007 data was collected between July in the calendar year 2006 and June 2007.

[20] Parental Medicaid eligibility threshold steadily increased in New York while remaining stable on average in other seven states. One case of State Medicaid expansion is in Iowa, where the monthly parental threshold for Medicaid increased from $1,065 to $1,268 per household in 2007.

projected revenue particularly at the local level, where the governments have limited resource to draw upon such as taxing, saving or borrowing. While the total revenue and expenditure trend converges over most of the sample period, there is a small surplus created between 2005 and 2006. The main objective of the first analysis is to empirically test the presence of substitution effect due to this surplus. In other words, I test whether spending in other categories changes before and after the cap policy among the affected counties, relative to changes in the same outcome among an appropriate control group. The treated group in this analysis are 45 counties that actually experienced negative growth in Medicaid spending after 2005. The 12 counties that did not benefit from the Medicaid mandate relief policy are used as the control group in the primary analysis using county-level variation within New York State[21].

I assess impacts on total non-Medicaid spending, debt and reserve fund balance. Total spending excluding Medicaid, debt and reserve balance are measured in both annual growth rate and per capita amount(in 2010 constant dollars). To further examine whether there were budgetary spillover effects on each spending category, I use category specific expenditure measured by annual growth rate, per capita level of expenditure, and share of total expenditure as the dependent variables. The budgetary spill-over effects will be analyzed using the following equation,

$$\mathrm{Y}_{\mathrm{c,t}} = \alpha + \sum_{p(t)\neq -1} \beta^{p(t)} D_{c,t}^{p(t)} + \mathbf{X}_{\mathrm{c,t}}\Gamma + \delta_{\mathrm{c}} + \lambda_t + \varepsilon_{\mathrm{c,t}} \quad (1)$$

where $D_{c,t}^{p(t)}$ is an indicator of lead and lag from the first year of reassessment for each county *c* in year *t*. The omitted time period is 2005 or one calendar year before the policy was

[21] These unaffected counties are Monroe; Hamilton; Putnam; Tioga; Tompkins; Allegany; Erie; Lewis; Onondaga; Rensselaer; Westchester and Wyoming.

implemented. $p(t)$ is an index of periods relative to the start of annual reassessment and period 0 indicates the first year when the counties were effectively affected by the cap policy. We would expect the spending reactions to occur immediately in 2006 if the budget officers of each administrative unit head and the governing board at the county level were well informed and convinced of the effectiveness of this sudden change in Medicaid budget. Given that most of the deliberation at the county level budgeting process happens in the latter half of county budget cycle — which is equivalent to a calendar year, if there is an immediate effect, such effect will be picked up by β^0. Unless the counties choose to save the extra monies in their unreserved fund, which I do not find any evidence on, the spillover effect should appear immediately after the cap is enacted. The coefficients on the lagged indicators, $\beta^{p(t)}$, are the estimates of differential change in outcomes $Y_{c,t}$ between the counties that experienced a reduction in Medicaid spending and those that did not relative to 2005.

A rich set of controls are also included in the vector of predetermined covariates, $\mathbf{X_{c,t}}$ that are suspected to commonly affect changes in spending of other categories. Factors that are expected to commonly affect the demand for relevant public services include median household income, population as well as population growth rate and county poverty rate. Voter characteristics such as the share of female, black and population over the age of 65 from the U.S. Census are also included. County unemployment rate is also included to capture the local business cycle, and particularly to control for the potential effect of great recession[22]. In addition, state and federal aid specific to spending category are added, since the determinants of demand

[22] As an assumption of the identification strategy for this study, a covariant balance test will be followed in order to confirm that the controls in $\mathbf{X_{c,t}}$ vary by each outcome.

for each public service will be different[23]. County fixed effects are included to control for unobserved heterogeneity across counties that do not vary across time. The effect estimates may be biased if there are time-variant confounders specific to counties in the error term. Such factors may include sudden changes in county officials' policy priority and local civil movements that affect county-level budget decisions. However, given the infrequent turnover of local officials in most upstate counties, this may be less of a concern. Finally, the reported standard errors are bootstrapped with 1,000 iterations, which shows to be more conservative than clustering the standard errors at the county level, given the small number of treated and control units.

5.2 Impact on Property Tax Relief: Empirical Models

New York county governments heavily rely on local property tax to finance their general fund expenditure[24]. In response to the 2005 Medicaid cap policy, county governments can either simply reallocate their limited resources to other spending categories or reduce property tax (while it is also possible that they reduce their debt or do nothing). Assuming that I find no significant changes in their total spending, the county governments may adjust their own source revenue in response to the cap policy. I test this with both county-only and non-county overlapping property tax rate as the dependent variables to check if there were any noticeable responses in other school districts or cities unaffected by the cap policy. I follow the standard approach of difference in differences to test for such heterogeneous effects, using equation (2),

$$\mathrm{y}_{\mathrm{c,t}} = \theta_0 + \theta_1 Post_t Treat_{c,t} + \mathbf{X}_{\mathrm{c,t}}\Upsilon + \delta_{\mathrm{c}} + \lambda_t + t\mu_c + \varepsilon_{\mathrm{c,t}} \qquad (2)$$

where $Post_t Treat_{c,t}$ represents an interacted dummy of post-intervention which will be one for years 2006 until 2010 and affected 45 NY counties and zero for years before 2006 and 12

[23] Intergovernmental transfers such as state aid or federal transfers with binding legal mandates may also affect spending level in a specific budget category which is why intergovernmental transfer is used for each category separately for different dependent variables.

[24] Property tax revenue accounted for 27.7% of NY counties' total own-source revenue, on average in 2010.

unaffected counties. Coefficient θ_1 indicates the slope differences between before and after the treatment, or whether there is an increasing or decreasing trend in spending in the particular category over time. The underlying idea is that each county just before and after 2006 are considered to be similar on average, which allows us to interpret any changes in the property tax rate during the post period as a consequence of the New York Medicaid cap policy.

In addition, I use the same difference in differences and event study approaches, to see if there were any differences in county-level property tax rates between the treated and comparison group, relative to the reference period in the absence of the cap policy. The event study estimates are reported in Table 6 and Figure 5 that illustrates the coefficients $\beta^{p(t)}$ for each sample period from equation (1). ${D^{p(t)}}_{c,t}$ denotes an indicator for county c being affected by the Medicaid cap policy in period $p(t)$, relative to the omitted reference year, which is one year before the Medicaid policy was enacted. This allows to check for the potential lagged effects of changes in property tax rate.

The potential fiscal impacts of the mandate relief may also vary across counties with different relative burden of Medicaid cost, relative demands for other public services, as well as the relative burden on its property tax levy. To account for the heterogeneity in Medicaid spending, I stratify the sample by growth rate of Medicaid expenditure, poverty rate and Medicaid enrollment, the results of which are reported in Panel B in Table 7. Through these subgroup analyses, we would expect to see larger property tax relief in counties that spent more than the median level of Medicaid expenditure than counties that spent less than the median amount. Similarly, we can suspect that the fiscal responses would be different between counties that experienced a greater decline in Medicaid spending between 2005 and 2006 than counties that did not benefit as much from the Medicaid cap policy. In addition to the event study analysis, I also use equation (3) to check for the heterogeneity effect across counties with different amount of savings in Medicaid costs,

$$lnY_{\mathrm{c,t+x}} = \alpha_0 + \alpha_1 lnMedicaidsaving_{c,t} + \mathbf{X}_{\mathrm{c,t}}\Upsilon + \delta_{\mathrm{c}} + \lambda_t + \varepsilon_{\mathrm{c,t}} \quad (3)$$

where $lnMedicaidsaving$ indicates the difference in linear projection of county Medicaid spending in the absence of the cap and the actual expenditure. Estimate of coefficient α_1 captures how much Medicaid saving would lead to changes in ETR after the cap. The effective tax rate is lagged by 1 to 3 years, following the standard approach of accounting for time lags in government fiscal response as well documented in the literature of public finance: The general idea is that there often are time lags for governments to respond to external macroeconomic shocks through fiscal and monetary policies. Gramlich (1987) in his seminal work on sub-national fiscal policies provided that there may be different types of lags — including a lag in recognizing the presence of a shock and the need to respond, in addition to lag in observing the effects of a responsive policy action. A more recent empirical study by Lutz (2008) found that there are two to three-year lags on average for property tax revenues to respond to local macroeconomic changes such as changes in housing prices. Such finding also seems applicable to this project since county governments may not be able to and/or willing to immediately change the property tax rate in response to the adoption of the Medicaid cap.

The key challenge for this study is that we cannot observe both potential outcomes for the same county, before and after the adoption of Medicaid mandate relief. The local Medicaid cap is a universal state policy that plausibly affects all counties in New York, which means that we need a reasonable comparison group of local governments that were unaffected by the policy to corroborate evidence for a causal argument. One of the main threats to internal validity of the within NY estimation is the presence of any other coincidental external event in 2006 that may have unevenly affected the fiscal behaviors at the county level. There is no such incident found between 2005 and 2007, so the immediate impact on both other spending and property tax rate may not be confounded. However, if there is a reason to believe that there might be lagged effects particularly on property tax rate, the NY School Foundation Aid reform and the Great Recession between 2007 and 2009 may be the potential confounders. Particularly in 2007, the New York State Education Budget and Reform Act of 2007 consolidated existing 30 school aid categories and streamlined the original formula to target additional aid resources to schools based on education needs. According to the state's projection, the foundation aid virtually led to an increase in school aid by approximately $1.1 billion each year. Therefore, it is worth including a reasonable comparison group in order to see whether any effect on property tax rate during the post period are purely due to the Medicaid mandate relief and not confounded by the school aid reform or the financial crisis.

Therefore, in the following analysis, I use effective property tax rate (ETR) of overlapping tax levying localities within a county area as the counterfactual. The NY Office of Real Property Tax Services annually reports both county-specific and county-wide full value property tax rate that are based on combination of levies across overlapping jurisdictions (city, town, village, school district and certain special districts) within a county-area. I first use the non-county ETR, i.e. the difference between county-wide property tax rate and county-specific property tax rate as the outcome variable using equation (1) and (2) for falsification test. Then I using the ETR of non-county tax levying localities within a county as the counterfactual of the county-specific ETR, I estimate the average treatment of the policy on the 45 affected counties at the county-area level with county fixed effects.

To test for the generalizability of the effects found in NY, I identify the impact of Medicaid mandate relief on property tax relief, using counties from comparable states as comparison units. I use two measures of outcome variable from two different datasets to utilize the variation across states. In the first analysis, I use property tax levy as the alternative measure of dependent variable and a larger pool of comparison counties. The dependent variable is measured as the log of properly tax levy per capita (in 2010 constant dollars) amount at the county level. The final sample include 1,309 counties in 21 states with Medicaid mandate. I also conduct subgroup analysis of 1,117 counties in 19 states with no property tax rate or levy limitations.

In the third analysis, I use effective property tax rate from six states, where counties have the similar role in levying the property tax. Among the six states, Iowa maintained the local Medicaid requirement throughout the sample period, while the counties in five other states were never required to share state Medicaid costs until 2010. I also include New Jersey which requires its counties to share the operating costs for county nursing facilities. New Jersey does not allow counties to have direct taxing authority, but still establishes the property tax rate and conduct assessments. New Jersey counties are included in one of the models where the comparison units continue to contribute to non-federal Medicaid costs.

For both analyses, the following equation (4) is used to estimate the property tax effect on the NY counties relative to various comparison counties,

$$\mathrm{Y}_{\mathrm{c,s,t}} = \rho_0 + \rho_1 Post_t Treat_{c,s,t} + \mathbf{Z}_{c,s,t}\Gamma + \mathbf{A}_{s,t}\Pi + \sigma_{\mathrm{c}} + \tau_{\mathrm{t}} + \varepsilon_{c,s,t} \quad (4)$$

where vector $\mathbf{Z}_{\mathrm{c,t}}$ includes county-level socio-economic variables such as county unemployment rate, growth in housing price, population growth, log of median household income as well as demographics (share of female, black and senior population) that may affect demand for public services. County level fiscal variables include log of own source revenue excluding property tax, log of state and federal aid, log of county revenue exported to other local governments. In order to account for predetermined heterogeneities across states, a vector of time-variant state controls $\mathbf{A}_{s,t}$. These include the growth rate of gross state product and dummies of industry structure (Finance, agriculture and durables) are included in the model as well. State Medicaid eligibility measured by parental Medicaid income threshold is also included to capture any state initiated expansion in Medicaid provision. County fixed effect is also included to control for time-invariant differences across counties, while we also test to see if the results are sensitive to including state fixed effect. Year dummies are included to account for any unobservable heterogeneity that is common across counties in a specific year. The estimates are also weighted by county population. The coefficient of interest is ρ_1, indicating the difference in property tax levy per capita and effective property tax rate before and after the Medicaid mandate relief between counties in NY State and other comparison states. The standard errors are adjusted using block bootstrap estimation to correct for auto-correlation (Bertrand et al 2003). If there is a parallel trend in property tax between both NY and control counties before 2006, we can argue that the average differences in the outcome during the post period are due to the Medicaid mandate relief.

6. Results and Discussion

6.1 Estimates of Expenditure Spillovers

Table 3 shows that total spending excluding Medicaid expenditure is unchanged, as well as debt and reserve fund balance using an event study framework. More importantly, there are no differences in total expenditure excluding Medicaid after the cap (relative to the omitted year, 2005) among the affected NY counties relative to one year prior. This finding is robust to the inclusion of a comparison group within NY, i.e. 12 counties that did not benefit from the cap policy. This finding holds when using the level of spending measured by log of per capita 2010 constant dollar instead of the first difference of logged values as the dependent variable.

Meanwhile, there is no evidence of significant change in borrowing behaviors, but some evidence of decline in unreserved fund balance among affected NY counties in 2010, relative to the other NY counties that did not benefit from the mandate relief. Nonetheless, while the budgetary spillover effects of the mandate relief is expected to occur in the short term, we do not observe any short term changes in spending, borrowing or saving behaviors among affected NY counties.

Panel A in Figure 2 shows that the average annual growth rate in total spending excluding Medicaid was higher than total spending between 2005 and 2007. This suggest that counties may have decided to partially redirect extra monies generated since 2005 to provide other public services instead of borrowing or saving. Overall, there seem to be compositional changes without a significant shift in the county budget constraint right after the cap was implemented

Such compositional changes are reported in Table 4 to Table 5. The single category where the level of spending significantly declined in budget in the year of policy enactment is general government expenditure. We observe an increase in miscellaneous other spending category. This increase however is offset by a decrease in general government spending by 81.57 dollar among 45 affected counties in 2006, relative to one year prior and the comparison counties.

We can also initially observe a positive growth in spending on social services other than Medicaid, such as financial and medical assistance in 2006, the change in level of which is statistically insignificant. Combination of these changes may partially explain why we do not observe any significant change in non-Medicaid total spending. Such finding is supported by the results when using share of each category spending out of total budget as the dependent variable instead of level of spending. An increase in the share of miscellaneous spending by 3.3% point is offset by a 4.7% point decline in the portion of general government spending out of total expenditure.

The county governments may have initially substituted away from Medicaid and redirected their limited resources to local public services such as financial assistance where the budget has been constantly declining during the pre-cap period. On the other hand, the fact that there is no significant evidence of change in other spending categories after 2007 leads us to the next question of whether the mandate relief policy may have been translated into a reduction in county-level property tax rate.

6.2 Effects on Property Tax Relief: Using Variations within New York

The average annual growth rate in counties' property tax levy was approximately 7 percent between 2001 and 2005, while the growth rate considerably declined in 2006 to 3.3 percent. Panel C in Figure 1 is a plot of the average county-specific effective property tax rate across 57 New York counties. Although we do not observe a discontinuity in property tax rate at the cutoff year, there seems to be some time lag in the response.

The difference in differences estimates in Table 7 show evidence of decline in both property tax levy and county-only effective property tax rate among the 45 affected NY counties, relative to the 12 unaffected units. These findings are corroborated by the event study estimates reported in Table 6 and Figure 5. The statistically insignificant estimates of β during the pre-cap period shows that county-only property tax rate trended similarly among the treated and untreated NY counties before 2005, which validates the underlying assumption of the difference in differences analysis. The results in columns (1) and (2) of Table 6 show strong evidence of an immediate and significant decrease in county-only effective property tax rate and property tax levy from 2007, one year after the policy was adopted. Estimates in column (4) are the coefficients of time dummies used for robustness check: Non-county property tax rate, i.e. county-wide overlapping tax rate minus county-only tax rate is used as the outcome variable that should not be affected by the cap policy. There is a notable differential trend of lower non-county property tax rate among the treated counties relative to the 12 control counties before 2006. However, the results in column (4) yield no evidence of a reduction in non-county local property tax rate during the post period. This suggests that the property tax relief impact was unique to the counties and not common across other local governments in NY. Figure 3 also shows the event study estimates in graphs.

The NY State's Real Property tax law section 900 stipulates that the board of supervisors in each county levies the property tax for the upcoming fiscal year, no later than the last day of each calendar year. Property tax rates are typically set at the final stage of the budget process after the appropriation resolution is adopted. The Medicaid mandate relief was enacted in October 2005, which makes it highly unlikely for the potential benefits to be reflected in the 2006 levy rate. The need to account for time lags in government fiscal response is well documented in the literature of public finance: The general idea is that there often are time lags for governments to respond to external macroeconomic shocks through fiscal and monetary policies. Gramlich (1987) in his seminal work on sub-national fiscal policies provided that there may be different types of lags — including a lag in recognizing the presence of a shock and the need to respond, in addition to lag in observing the effects of a responsive policy action. A more recent empirical study by Lutz (2008) found that there are three-year lags on average for property tax revenues to respond to local macroeconomic changes such as changes in housing prices. Such finding also seems applicable to this project since county governments may not be able to and/or willing to immediately change the property tax rate in response to the adoption of the Medicaid cap.

The extent to which counties adjust property tax rates in response to the Medicaid mandate relief may also vary across counties depending on their relative burden of Medicaid cost. Table 7 reports how the pre and post differences in property tax rate within affected counties can be associated with the Medicaid cap policy. The difference in differences estimates shows that the NY counties reduced the effective property tax rate by 0.652 mills rate on average, relative to the pre-period[25]. The magnitude of this effect is also economically significant, since it is approximately a 8% decline from the average ETR among affected counties in 2005. The property tax relief effect also remains significant when I limit the post period to 2006-2008: The average treatment effect of the cap on county level effective property tax rate among the affected NY counties is -0.662 which is statistically significant at the 10% level. The findings also is robust to use of alternative measure of outcome, log of property tax levy per capita. The level of property tax levy was significantly lower by 6% during the post period among treated counties relative to the unaffected NY counties.

[25] In the meantime, sales and use tax revenue did not show any significant drop associated with Medicaid spending after the cap was introduced. Also, there is no significant change in the amount of state aid received by counties during the sample period.

On the other hand, I find that there is no evidence of change in property tax rate among other localities within the county area that should not be affected by the cap policy, during the post period. In fact, the event study results show positive coefficients estimates of non-county property tax rate in 2006 and 2008. The DD estimates in column (5) and (6) in Panel A of Table 7 suggest that the property tax relief is unique to county governments and not found among other level of governments within the county boundaries. When using non-county property tax rate as the counterfactual potential outcome of county-specific property tax rate, we find larger and statistically significant estimates of property tax relief for counties, as reported under column (4) in Table 6 and Panel A of Table7.

The subgroup analyses results in Panel B of Table 7 depict the differential effects across counties in terms of the level and growth rate of Medicaid expenditure. As expected, we observe a significant decrease in property tax rate by 0.779 mills rate among counties that have higher than state-wide median poverty rate at the county level. I do not find evidence of property tax relief among relatively affluent counties with lower than median poverty rate and Medicaid enrollment. The estimated effects also seem larger among counties with higher growth rate in Medicaid spending than counties with lower than median growth rate (-1.431 versus -0.605). However, since the confidence intervals of the two estimates overlap, we cannot reject the hypothesis that the effects are similar between the two groups. In other words, there is no evidence of larger property tax relief in counties that spent more and experienced a greater drop in Medicaid outlay, relative to counties that spent less and experienced smaller decline in Medicaid spending in the year of the policy enactment. Nonetheless, both groups show a significant drop in property tax rate during the post period. The results in Panel C provide support the finding of a two year lagged effect of property tax relief among NY counties. The coefficient of the interacted term between a post dummy and the log of Medicaid savings amount (measured as the logged difference between the estimated counterfactual and actual Medicaid expenditure) is statistically significant and negative at the 10% level. This suggests that a one percent increase in Medicaid savings from the cap policy led to 0.384% decline in effective property tax rate on average during the post period.

One of the key underlying assumptions of the analyses using within-NY variation is the absence of historical threats during the post period that may have differential affect on county fiscal outcomes and bias the effect estimates. However, there are two external events during the post period that may potentially pose threat to internal validity of these estimates using variations within NY – the NY School Finance reform implemented in 2008 and the Federal Stimulus package introduced in 2009. The School Finance Reform was the NY state government's plan to provide additional $1.7 billion to eligible school districts starting from 2008 by introducing a new foundation aid formula[26]. If the provision of foundation aid successfully led to property tax relief among affected school districts, such decision may have spillover effects on revenue raising behaviors at the county level. However, this may not be a major concern since we do not observe any significant change in non-county local property tax rate that includes the school district's portion during the post period among affected counties. When comparing the pre and post outcomes of 57 counties without a comparison group, we observe evidence of an increase in non-county property tax rate, which albeit being statistically significant, has little economic significance when compared to the 2005 baseline level of 24 mills rate. Although the estimate becomes statistically significant and negative in 2010, this does not bias our estimate of the cap policy's effect. The potential lagged effect should typically last for two to three years according to the public finance literature (Gramlich 1987; Lutz 2008). In fact, the average effective

[26] While the Foundation aid was designed to phase-in over four years, funding froze since 2009.

property tax rate for non-county localities do not show any significant decrease between the first post period of interest, which is 2008.

Another event that may confound our effect estimates in later post years is the federal stimulus package in 2009 after the great recession. The federal government included $2.7 billion in the federal stimulus package as part of the Federal Medical Assistance Percentages aid to support the local governments in New York State. However, the federal aid may not lead to any bias in the estimates, as the federal decision to provide fiscal relief is exogenous to the New York State government's decision to introduce Medicaid cap policy in 2005. Also, the county-only property tax rate started declining from 2007, which is two years before the stimulus package was introduced.

6.3 Robustness Check: Effect on Property Tax Relief Using Variations across States

In order to account for such potential confounders, the next step is to introduce an appropriate comparison group and check whether the previous findings from the within-NY estimation holds. A difference-in-differences and event study estimator is employed to estimate the average treatment effect on the treated counties, using two datasets to find comparable counties outside of New York State.

First, I use effective property tax rate of comparison counties from seven states outside of New York State. The final sample includes counties from five states that never required their localities to share Medicaid costs until 2010, as well as counties in Iowa and New Jersey that maintained state mandates for counties to share the state Medicaid costs. Iowa, has a relatively less stringent mandate, which requires its counties to pay 4.3% of the total state Medicaid cost to the state[27]. Introducing these comparison units from outside of New York State can be useful for addressing the concern of the great recession being a potential confounder that biases the within-NY estimates. The property tax rate level may vary across states, with New York having the highest property tax rate, yet this may not affect the main analysis since we are mainly interested in estimating the slope difference before and after the cap policy.

As an alternative to using the limited pool of states where county-specific effective property tax rate is available, I use property tax levy growth from the Annual Survey of State and Local Government Finances. To cope with any potential heterogeneity in housing market fluctuations, I control for annual growth in housing price index, provided by the Federal Housing Finance Agency in addition to county unemployment rate and the same set of county and state level covariates used in equation (4). One caveat of using the levy growth as the outcome variable is that it is not the optimal measure for cross-jurisdiction comparison, unlike effective property tax rate since the property tax levy may not be specific to county if the county distributes the revenue to other localities including school districts. However, this measure can be still used to include a more comprehensive pool of counties in states that maintained local Medicaid mandates and particularly those without restrictive tax limits.

The key assumption remains the same for both analyses: Ideally, there should be no differential trend in property tax burden over time between affected NY counties and comparison counties, in the absence of the spending cap. In other words, there should be no heterogeneous

[27] Other potential comparison states that have significant local Medicaid requirement include Arizona, New Hampshire, California and North Carolina. However, North Carolina initiated relieving the local requirement gradually from 2005 and eventually eliminated the mandate by 2010, replacing it with the options to remit county level sales tax revenue to the state government. California is an exceptional case with restrictive property tax limitations and New Hampshire counties do not have the authority to levy property tax.

policies across counties during the sample period that would alter the county property tax rate, other than through the indirect channel of the mandate relief[28].

The DD results using property tax levy growth and a larger pool of comparison counties outside of New York is reported in Tables 8. The estimates show consistent evidence of significant decline in property tax levy growth among affected New York counties, relative to various groups of comparison counties. It is important to address the potential confounding effects of property tax limitations since counties in states with property tax limitations (particularly rate and levy limits) may have limited discretion to increase their property tax revenues. The effect estimates vary between -0.077 and -0.084, with higher estimates when using subgroup of counties in states without restrictive rate or levy limits that should be more comparable to NY counties. The effect estimate using more comprehensive model using all states with mandate may be underestimated due to the inclusion of counties in states that restrict the growth of levy amount or nominal tax rates which are less similar to the treated counties in New York.

In order to test whether the parallel trend assumption holds and to check for the dynamic effect of the cap policy, I use an event study framework. An event study analysis also allows us to capture the timing of the change in the property tax rate relative to the timing of cap enactment. In Table 9, none of the pre-period annual coefficients show to be statistically significant in any of the models. The event study estimates consistently show evidence of a reduction in treated counties' property tax rate between 2007 and 2008, the result of which is robust to the selection of comparison group. We find negative coefficients on post period indicators from 2007 when comparing NY counties to counties in states that maintained their Medicaid mandate and had no

[28] However, if there are time-variant unobserved components that may affect changes in the property tax rate, this model may still suffer from omitted variable bias.

restrictive property tax limitations. The suggestive evidence of lagged property tax relief is also evident and consistently found across various subgroup analyses using event study estimator. Figure 6 also shows the event study estimates in graphs.

These findings are corroborated when using variation in effective property tax rates across counties in different states. As shown in Table 10, the DID estimates provide strong evidence of decline in effective property tax rates between 2006 and 2010 in New York counties compared to various combinations of comparable counties in seven other states. We find statistically significant effects that are robust across samples using different combinations of comparison groups. The results tell us that NY counties had ETR that are lower than comparable counties by 0.644 to 0.827 during the post period, which is similar to the event study estimate for the first post period (-0.548) using within NY variation. The estimate of key interest is under column (2) that suggests affected New York counties had lower effective property tax rate by 0.827, relative to counties in New Jersey and Iowa that continued to pay their share of Medicaid costs to the state government throughout the sample period. All coefficients of the interacted term reported in Table 10 have significant negative signs in every subgroup analysis[29].

Similar to the previous analysis using property tax levy in counties across states, one concern about the choice of seven comparison states is the potential confounding effects of property tax limitations. Two states - Iowa, and Kentucky - have rate limits on overall county property tax rates[30]. Iowa is the only state that has assessment limits that constrains the annual rate of growth of assessed value. Assessment cap in general, however, is least restrictive

[29] An exception is where the comparison units counties that are not required by the state to share the cost of Medicaid. The effect estimate from this subgroup analysis still shows to be negative although, statistically insignificant. Nonetheless, we observe signs of reduction in property tax rate among affected New York counties from 2008, relative to counties in these five states in the event study results.

[30] Kentucky is the state with the most restrictive tax rate limit of 0.5% as well as a cap on the growth rate of the property tax rate to a maximum of 4%.

compared to tax rate limits and does not effectively deter increases in the tax rate, but rather is designed to restrict incidental tax increases. In addition, Iowa has a relatively permissive annual cap, which is 4% for residential and agricultural parcels, but no limits for commercial and industrial properties. Virginia and Kentucky are the two states with limitations on property tax levy(or revenue) increase, which can be less restrictive than capping the level of nominal tax rate. Nonetheless, no comparison state included in the sample has any legislative changes during the study period that may have directly affected county level property tax rates. The event study estimates in Table 11 reveals that there were no differential trends in property tax rate between New York counties and the comparison counties in states with property tax rate limitations before enactment of the cap policy. Both event study and DID estimates show to be robust to the selection of counties in states with and without restrictive tax rate limits. Overall, the event study estimates corroborates the difference-in-differences results of a reduction in treated counties' property tax rate between 2008 and 2010, the result of which is robust to the selection of comparison group.

7. Conclusion

This paper shows estimates of the impact of intergovernmental fiscal policies on local fiscal behavior. While the public finance literature has predominantly focused on testing the crowd out effects of growing expenditure, the potential spillover effects of a large reduction in spending should also receive an in-depth scrutiny. In this paper, we suggest that the local fiscal responses to a decrease in the growth of one major spending category may be different from the typical reaction to a steady increase. This study examined New York counties in the United

States to see how a higher-level government's assumption of local spending on a large public health insurance program may have affected budgetary decisions and property taxation.

In this paper, there is no evidence of short term changes in the borrowing or saving behaviors among affected NY counties after the mandate relief. In response to reduced Medicaid burden, county governments instead, redirected small portions of their monies to other miscellaneous operational functions over the first two post periods. Nonetheless, this study shows no evidence of a net increase in their total outlay after the cap. The NY county governments did not become more generous in spending when the state government lowered counties' Medicaid spending.

This finding complements past studies on federal Medicaid expansion crowding out other public services (Baicker 2005; Kane et al. 2002). This study provides evidence of asymmetric response in a budgetary trade-off in the short term: While previous work finds federal-mandated increases in Medicaid spending reducing spending on other categories, I do not observe a state-mandated decreases in Medicaid spending leading to a positive spillover on other categories. When coupled with slower growth in total spending, I do not find any evidence of budgetary spillover effect in the longer term, and instead find evidence that the cap policy led to property tax relief.

When using variation across counties within NY, the effect estimates suggest that the NY counties experienced a decrease in property tax rate by one mill rate, on average, after the policy was implemented. There is no statistically significant evidence of heterogeneous effects between counties that were presumably expecting to benefit more from the policy and those that were less affected. However, I find consistent evidence of a decline in NY counties' property tax rate after enactment of the cap or at least potential lagged effects on county property tax rate from event study results.

These findings are supported by difference-in-difference estimates that show by 6.6 to 8.1% decrease in effective property tax rate or 6% reduction in levy per capita among NY counties during the post-cap period, relative to unaffected counties in other comparable states. I also find that one percent increase in Medicaid savings led to 0.23 percent reduction in ETR, two years after the cap was enacted. These findings suggest that a higher level government's decision to assume parts of local expenditure also has the potential to provide property tax relief at the local level. Overall, this study shows that reallocation of limited sub-national government's resources or fiscal responsibility may have spillover effects on revenue raising behaviors as well as budgetary decisions.

However, there are numerous limits to this study in its current form, primarily due to data limitations. An alternative way would be using a larger donor pool of counties from states that never had the cost-sharing component as a comparison group and using synthetic control method to address the same question. There also may be other ways to explain why we do not observe an immediate response in local property tax rate, other than mere time lags.

Finally, there are other related questions that are worth addressing but were not within the scope of this research. A state government's takeover of local fiscal responsibilities may lead to an increase in the need to raise tax levy at the state level, thereby canceling out the effects on local tax relief. Therefore, the next step would be to check whether the policy affected the net tax burden on households in treated regions. One can also test whether this policy led to changes in the composition of the tax burden between different levels of governments.

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Table 1. Summary statistics of New York counties

Panel A. New York Counties

	Mean	Std.dev	Min	Max
Property tax rate 1 (County only)	6.89	3.60	0.20	37.20
Property tax rate 2 (Countywide)	31.86	6.19	14.70	49.50
Δ Log(Property tax levy)	0.04	0.132	−0.33	0.71
County Medicaid spending ($1,000)	38,200	59,900	441	370,000
Annual growth in county Medicaid spending	0.16	0.19	−0.86	3.58

Panel B. Treated and control units within New York

	Treated	Control	P-value of difference
County characteristics			
Medicaid budget per capita (2010$)	208.88	180.67	0.001***
Property tax rate (County only)	7.12	6.44	0.164
Property tax / Total revenue (%)	19.40	18.14	0.105
Non-Medicaid total budget per capita (2010$)	1,103	1,286	0.000***
Debt per capita (2010$)	40.222	48.496	0.193
Undesignated reserve per capita (2010$)	145.57	141.51	0.860
Share of Medicaid enrollee population (%)	13.90	12.09	0.000***
County covariates			
Unemployment rate	5.61	5.36	0.116
Population	164,396	299,137	0.001***
Population growth rate	0.441	0.000	0.554
Share of senior population (%)	14.55	14.04	0.051*
Share of female population (%)	51.52	51.71	0.095*
Share of black population (%)	3.98	5.83	0.000***
Blind or disabled/Medicaid enrollment (%)	4.8	5.6	0.127
Poverty rate (%)	11.34	10.45	0.010**
Median household income ($)	40,451	43,427	0.012**
Log(intergovernmental transfer)	11.03	11.32	0.020**
Log(own source revenue minus property tax)	11.00	11.13	0.410

Note: Figures in second and third columns in Panel B represent mean characteristics of counties before 2005. *** p<0.01, ** p<0.05, * p<0.1

Table 2. Summary statistics of counties in New York and comparable states

	New York			Other seven comparison states		
	Pre	Post	P-value	Pre	Post	P-value
Property tax / Total revenue (%)	17.9	19.4	0.018**	26.4	26.1	0.870
Effective property tax rate (per $1,000AV)	7.11	6.62	0.096*	2.97	3.18	0.000***
County covariates						
Unemployment rate	5.0	6.3	0.139	5.03	7.11	0.000***
Population	308,340	310,157	0.079*	65,679	69,489	0.246
Population growth rate (%)	0.30	-0.15	0.000***	1.37	0.83	0.000***
Share of senior population(%)	14.32	14.19	0.510	13.92	13.93	0.975
Share of female population(%)	51.76	51.32	0.000***	51.75	51.32	0.000***
Share of black population(%)	4.23	4.54	0.304	14.1	14.2	0.875
Poverty rate	11.8	13.2	0.000***	14.08	16.44	0.000***
ΔLog(personal income)	0.032	0.036	0.056*	0.039	0.037	0.212
Log(federal aid)	7.74	7.99	0.108	6.07	6.47	0.000***
Log(state aid)	10.91	11.05	0.189	9.08	9.31	0.000***
Log(own source revenue-property tax)	11.57	11.82	0.028**	9.49	9.77	0.000***
State covariates						
Medicaid income threshold($)	1,677	2,200	0.000***	679	843	0.000***
GSP growth rate	4.54	3.23	0.418	4.89	2.57	0.031**
Finance (Share of industry, %)	30.2	29.5	0.334	17.82	17.96	0.028**
Agriculture (Share of industry, %))	0.201	0.204	0.829	1.89	2.07	0.016**
Durables (Share of industry, %))	3.4	2.7	0.004***	8.24	6.85	0.000***
	All states with local mandates[31]			States with local mandates but no levy or rate limit[32]		
Δ Log(Property tax levy)	0.06	0.05	0.122	0.06	0.05	0.342
Unemployment rate	5.33	6.49	0.000***	5.27	7.01	0.000***
Population	146,968	146,053	0.895	186,366	194,261	0.493
Population growth rate (%)	1.25	0.52	0.000***	1.50	1.51	0.334
Share of senior population(%)	15.02	15.10	0.334	15.41	15.40	0.981
Share of female population(%)	51.53	51.09	0.000***	51.66	51.30	0.000***
Share of black population(%)	4.86	4.98	0.506	8.43	8.98	0.875
Poverty rate	11.55	12.68	0.000***	9.54	11.05	0.000***
ΔLog(personal income)	0.03	0.04	0.000***	0.04	0.03	0.000***
ΔLog(housing price growth)	1.73	0.83	0.000***	1.79	1.16	0.000***
Log(export revenue to local governments)	8.92	9.24	0.080*	6.81	7.02	0.043**
Log(federal aid)	6.48	6.91	0.000***	7.08	7.60	0.000***
Log(state aid)	8.62	8.97	0.000***	9.72	10.02	0.000***
Log(own source revenue-property tax)	7.72	9.09	0.000***	8.34	9.93	0.000***
Medicaid income threshold($)	876	1,074	0.000***	1,114	1,408	0.000***
GSP growth rate	4.92	3.64	0.000***	4.56	3.02	0.000***
Finance (Share of industry, %)	18.57	18.30	0.000***	19.38	19.36	0.873
Agriculture (Share of industry, %))	1.72	2.10	0.000***	1.31	1.45	0.000***
Durables (Share of industry, %))	9.17	8.11	0.000***	10.41	10.44	0.875

[31] The other comparison states with Medicaid local mandate are Florida; Hawaii; Illinois; Indiana; Michigan; Minnesota; Montana; Nevada; New Hampshire; New Jersey; New Mexico; North Dakota; Ohio; Oregon; Pennsylvania; South Carolina; South Dakota; Texas; Utah; Washington and Wisconsin.

[32] States without neither rate nor levy limit are Hawaii, Maryland, New Hampshire, Tennessee, Connecticut and Vermont. However, counties in latter two states do not have the authority to levy property tax. Among states with Medicaid mandates, those with either rate or levy limit are Florida; Hawaii; Minnesota; Oregon, South Carolina and Wisconsin.

Table 3. Event study estimates: Total expenditure, debt and savings

Panel A. Annual growth rate

	Δln(Total)	Δln(Total - Medicaid)	Δln(Debt)	Δln(Unreserved fund)
D^{-5}	−0.024	−0.028	−0.716*	0.475
	(0.033)	(0.049)	(0.376)	(0.824)
D^{-4}	0.006	0.011	−0.076	−0.550
	(0.033)	(0.040)	(0.346)	(0.421)
D^{-3}	−0.041	−0.048	−0.363	−0.205
	(0.032)	(0.036)	(0.294)	(0.728)
D^{-2}	0.001	0.008	−0.221	0.139
	(0.038)	(0.062)	(0.380)	(0.425)
D^{0}	−0.054	−0.052	−0.233	−0.489
	(0.038)	(0.043)	(0.237)	(0.319)
D^{1}	−0.015	−0.013	−0.273	−0.085
	(0.035)	(0.048)	(0.232)	(0.275)
D^{2}	−0.025	−0.031	−0.306	−0.261
	(0.037)	(0.042)	(0.235)	(0.397)
D^{3}	−0.067	−0.068	−0.230	−0.184
	(0.046)	(0.048)	(0.278)	(0.333)
D^{4}	−0.010	−0.006	−0.246	−0.705*
	(0.028)	(0.031)	(0.323)	(0.373)
Y^{-1} | affected	0.062	0.078	0.284	0.370
	(0.086)	(0.095)	(0.796)	(0.331)
D (2005)	−0.031	−0.031	−0.455	−0.264
	(0.027)	(0.029)	(0.858)	(0.215)

Note: N=570. 2005 is used as the reference year. Standard errors bootstrapped with 1,000 replications are reported in parentheses. All models control for observable and unobservable county characteristics. Estimates under columns (2) and (4) use twelve NY counties that did not experience a reduction in Medicaid spending between 2005 and 2006 as comparison groups. All estimates are from an event-study model, using equation (1) and are coefficient estimates of variable $D^{p(t)}$ that measure the differential change in outcomes between the treated counties and comparison counties in each $p(t)$, relative to period -1. *** p<0.01, ** p<0.05, * p<0.1

Panel B. Log of per capita amount (2010 constant dollars)

	Non-Medicaid total	Debt	Unreserved fund
D^{-5}	−0.042	−0.325	−0.172
	(0.064)	(0.307)	(0.491)
D^{-4}	−0.012	−0.075	−0.561
	(0.069)	(0.258)	(0.420)
D^{-3}	−0.032	−0.012	−0.513
	(0.071)	(0.325)	(0.350)
D^{-2}	−0.016	0.102	−0.036
	(0.062)	(0.234)	(0.278)
D^{0}	−0.028	−0.081	−0.273
	(0.049)	(0.203)	(0.273)
D^{1}	−0.011	−0.092	−0.531
	(0.046)	(0.215)	(0.335)
D^{2}	−0.007	−0.174	−0.169
	(0.058)	(0.239)	(0.283)
D^{3}	−0.035	−0.320	−0.252
	(0.048)	(0.207)	(0.286)
D^{4}	−0.033	−0.111	−0.793***
	(0.089)	(0.233)	(0.189)
Y^{-1} | affected	7.237	5.589	5.283
	(0.033)	(0.031)	(0.112)
D (2005)	−0.120	−0.292	0.527
	(0.079)	(0.338)	(0.283)

Note: N=570. Year 2005 is used as the reference year. Standard errors bootstrapped with 1,000 replications are reported in parentheses. All models control for observable and unobservable county characteristics. Reported estimates show the differential change in outcomes among 45 affected counties, relative to 12 unaffected NY counties that did not experience reduction in Medicaid spending after enactment of the cap policy. *** p<0.01, ** p<0.05, * p<0.1

Table 4. Event study estimates: Share of total expenditure

	Social Services (w/o Medicaid)	General Government	Community	Employee Benefits	Education	Economic Development	Safety	Transportation	Others
D^{-5}	0.006	−0.002	−0.001	−0.006	0.007	−0.004	−0.005	−0.011	0.016
	(0.009)	(0.019)	(0.002)	(0.008)	(0.005)	(0.006)	(0.006)	(0.013)	(0.018)
D^{-4}	0.010	0.005	−0.003	−0.011	0.007*	0.002	−0.002	−0.021*	0.019
	(0.008)	(0.019)	(0.003)	(0.008)	(0.004)	(0.003)	(0.006)	(0.011)	(0.014)
D^{-3}	0.009	0.006	−0.001	−0.008	0.003	−0.002	−0.003	−0.010	0.004
	(0.008)	(0.017)	(0.002)	(0.008)	(0.004)	(0.003)	(0.007)	(0.012)	(0.017)
D^{-2}	0.006	-0.017	−0.002	−0.004	0.003	-0.001	-0.008	−0.001	0.019
	(0.006)	(0.021)	(0.002)	(0.006)	(0.005)	(0.002)	(0.006)	(0.014)	(0.012)
D^{0}	0.011	−0.047**	−0.001	0.004	0.000	−0.001	−0.005	−0.001	0.033*
	(0.007)	(0.023)	(0.002)	(0.006)	(0.007)	(0.002)	(0.008)	(0.011)	(0.017)
D^{1}	0.004	−0.005	−0.001	−0.002	−0.001	−0.002	−0.006	−0.003	0.015
	(0.008)	(0.019)	(0.002)	(0.007)	(0.005)	(0.002)	(0.007)	(0.012)	(0.013)
D^{2}	0.008	−0.003	0.000	−0.004	0.004	−0.001	−0.011	−0.016	0.026**
	(0.008)	(0.019)	(0.003)	(0.008)	(0.005)	(0.002)	(0.008)	(0.013)	(0.012)
D^{3}	0.009	−0.001	−0.001	−0.004	0.004	−0.001	−0.007	−0.008	0.018
	(0.009)	(0.018)	(0.002)	(0.009)	(0.005)	(0.003)	(0.007)	(0.012)	(0.011)
D^{4}	0.006	−0.002	−0.000	−0.002	0.003	−0.001	−0.004	−0.004	0.019*
	(0.009)	(0.019)	(0.002)	(0.007)	(0.005)	(0.003)	(0.006)	(0.012)	(0.011)
Y^{-1} | affected	0.177	0.098	0.016	0.120	0.041	0.007	0.091	0.088	0.079
Constant	0.191	−0.693	0.024	1.176***	−0.332	0.244	−0.019	−0.126	0.542
	(0.389)	(0.922)	(0.093)	(0.310)	(0.268)	(0.179)	(0.271)	(0.461)	(0.793)

Note: N=570. Omitted year is 2005. Bootstrapped standard errors are in parentheses. All models control for observable and unobservable county characteristics. All reported estimates use twelve NY counties that did not benefit from the cap policy as comparison groups. Bootstrapped standard errors are in parentheses. N=570. *** p<0.01, ** p<0.05, * p<0.1

Table 5. Event study estimates: Level of total expenditure, per capita

	Social Services (w/o Medicaid)	General Government	Community	Employee Benefits	Education	Economic Development	Safety	Transportation	Others
D^{-5}	−18.450	8.348	−1.437	21.251	5.833	−3.882	−7.279	−6.721	30.465
	(15.183)	(34.191)	(2.624)	(39.798)	(10.548)	(6.440)	(8.864)	(25.910)	(43.687)
D^{-4}	−12.308	9.240	−2.585	4.896	10.228	2.367	−5.047	−19.161	25.925
	(12.470)	(37.542)	(2.582)	(30.572)	(7.966)	(4.582)	(9.330)	(18.932)	(30.899)
D^{-3}	−9.427	15.881	−0.967	7.687	5.007	−2.235	−5.369	−15.891	3.091
	(15.287)	(29.878)	(2.240)	(28.826)	(8.089)	(5.382)	(11.443)	(19.061)	(31.469)
D^{-2}	−7.095	−36.654	−1.637	−17.062	2.731	−1.866	−8.271	−4.211	26.937
	(12.257)	(58.004)	(2.344)	(16.046)	(7.752)	(3.521)	(7.524)	(20.374)	(30.120)
D^{0}	8.444	−81.577**	−1.263	3.523	−3.620	−2.827	−9.682	−4.596	61.898*
	(12.658)	(35.010)	(2.060)	(16.251)	(7.859)	(3.906)	(11.550)	(18.513)	(35.410)
D^{1}	−0.092	−32.290	1.036	−2.456	−4.150	−5.597	−14.829	−13.480	10.470
	(13.878)	(37.936)	(2.891)	(20.680)	(8.949)	(4.611)	(12.281)	(26.342)	(30.885)
D^{2}	8.198	−28.088	1.472	−14.497	2.483	−5.097	−21.214	−53.827	49.915
	(16.841)	(29.316)	(3.679)	(22.743)	(9.766)	(4.473)	(14.588)	(35.384)	(33.152)
D^{3}	16.410	−7.341	−1.549	−33.030	−2.356	−1.397	−15.488	−18.839	38.220
	(20.030)	(42.401)	(2.540)	(52.237)	(12.673)	(3.943)	(13.518)	(23.781)	(30.241)
D^{4}	13.651	−18.454	0.556	−39.308	−4.500	−1.205	−14.218	−3.990	26.866
	(25.904)	(38.491)	(2.898)	(52.244)	(16.047)	(3.607)	(11.427)	(22.345)	(29.241)
Y^{-1} | affected	244.26	141.02	23.20	170.98	58.16	11.17	134.80	124.11	189.56
Constant	2,590	1,539	267	5,580***	−956**	739**	404	1,593	2,639
	(1,688)	(2,232)	(173)	(1,896)	(405)	(335)	(562)	(1,946)	(1,868)

Note: N=570. Omitted year is 2005. Bootstrapped standard errors are in parentheses. All models control for observable and unobservable county characteristics. All reported estimates use twelve counties that did not benefit from the cap policy as comparison groups. Bootstrapped standard errors are in parentheses. N=570. *** p<0.01, ** p<0.05, * p<0.1

Table 6. Effect on county and local property tax rate: Event study estimates

	Outcome variables			
	Ln (Levy per capita)	County only ETR		Non-county local ETR
	(1)	(2)	(3)	(4)
D^{-5}	−0.096	−0.162	−0.839	−0.352
	(0.062)	(0.465)	(0.961)	(1.350)
D^{-4}	−0.048	−0.375	−0.141	−2.170**
	(0.058)	(0.412)	(0.494)	(0.944)
D^{-3}	−0.018	−0.610	−0.059	−1.154
	(0.054)	(0.413)	(0.673)	(1.044)
D^{-2}	−0.045	−0.323	0.763	−1.108
	(0.055)	(0.491)	(0.588)	(0.994)
D^{0}	−0.054	−0.543	−1.173***	0.032
	(0.052)	(0.387)	(0.449)	(1.175)
D^{1}	−0.104**	−1.313***	−1.358	−0.308
	(0.049)	(0.483)	(1.203)	(0.893)
D^{2}	−0.113**	−1.080**	−3.758***	−2.152
	(0.048)	(0.487)	(0.632)	(1.349)
D^{3}	−0.122**	−1.022*	−1.784**	−1.997*
	(0.061)	(0.605)	(0.702)	(1.031)
D^{4}	−0.118	−0.979*	−0.065	−1.706
	(0.072)	(0.561)	(0.650)	(1.223)
Y^{-1} | affected	7.781	7.980		24.097
	(3.850)	(3.937)		(3.929)
# Treated units	45	45	45	45
# Control units	12	12	45	12
Level of control units	County	County	Non-county localities	County

Note: All estimates are from an event-study model, using equation (1) and are coefficient estimates of variable $D^{p(t)}$ that measure the differential change in outcomes between the treated counties and comparison counties in each $p(t)$, relative to period -1. Omitted period -1 is calendar year 2005. All models control for observable and unobservable county characteristics. Estimates under columns (1), (2) and (4) are from analyses using twelve NY counties that did not benefit from the cap policy as comparison groups. Reported estimates under column (3) are from an analysis using ETR of other local jurisdictions within the county area as the counterfactual. Bootstrapped standard errors with 1,000 replications are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 7. Effect on county and local property tax rate: Difference in differences estimates

Panel A. County-only and local effective property tax rate

	Ln(Levy per capita)	County-only property tax			Non-county property tax	
	(1)	(2)	(3)	(4)	(5)	(6)
θ_1	-0.066^{**}	-0.652^{**}	-0.662^{*}	-1.418^{***}	-0.159	0.062
	(0.030)	(0.349)	(0.378)	(0.434)	(0.544)	(0.661)
$Y^{-1} \mid Treat = 1$	5.92		7.99			25.08
# Treated units	45	45	45	45	45	45
# Control units	12	12	12	45	12	12
# of years	10	10	8	10	10	8

Panel B. Subgroup analysis: Heterogeneity in Medicaid spending and poverty rate

DV: County-only ETR	Δ Medicaid spending		Poverty rate		Medicaid enrollment	
	(1) High	(2) Low	(3) High	(4) Low	(5) High	(6) Low
θ_1	-1.431^{*}	-0.605^{**}	-0.779^{*}	-0.699	-0.799^{***}	-0.436
	(0.766)	(0.287)	(0.404)	(0.526)	(0.288)	(0.695)
$Y^{-1} \mid Treat = 1$	8.288	7.682	9.493	6.423	9.127	6.695
# Treated units	23	22	23	22	23	22
# Control units	12	12	12	12	12	12

Note: Bootstrapped standard errors are in parentheses. All models in both panel control for observable and unobservable county characteristics. Reported estimates under column (4) in Panel A are from a DD analysis using ETR of other local jurisdictions within the county area as the counterfactual. High groups in Panel B (columns (1), (3) and (5)) refer to 22 counties that had higher than or equal to the median value of outcome among treated 45 NY counties in 2005. *** p<0.01, ** p<0.05, * p<0.1

Panel C. Interaction with estimated Medicaid savings from the cap policy

	County-only ETR			Non-county local ETR	
DV:	Ln(ETR)_{t+1}	Ln(ETR)_{t+2}	Ln(ETR)_{t+3}	Ln(ETR)_{t+2}	Ln(ETR)_{t+3}
			Affected 45 New York Counties		
α_1	−0.595	−0.229*	−0.326**	0.064	0.066
	(1.043)	(0.131)	(0.165)	(0.075)	(0.080)

Note: N=450. Only 45 treated NY counties were used in this analysis. All models include county level covariates as well as year and county fixed effects. α_1 indicates the coefficient of Ln(Medicaid saving) in equation (3). Bootstrapped standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 8. Difference in differences estimates with different control groups

	States with local Medicaid mandate			*All states*
DV: ln (Levy per capita)	(1) All states with mandate	(2) No rate or levy limit	(3) No levy limit	(4) No rate or levy limit
ρ_1	-0.014	-0.081^{**}	-0.084^{**}	-0.077^{**}
	(0.023)	(0.037)	(0.032)	(0.035)
Υ^{-1}	5.33	5.63	5.51	5.34
# of observations	6,109	2,593	1,373	7,276

Note: Standard errors clustered at the county level reported in parentheses. Number of treated counties in New York is 45. The number of comparison states in each column are 21, 8, 5 and 19 respectively.*** p<0.01, ** p<0.05, * p<0.1

Table 9. Levy growth: NY versus comparison counties from other states

DV: ln (Levy per capita)	*States with local Medicaid mandate* (1) All states with mandate	(2) No rate or levy limit	(3) No levy limit	*All control states* (4) No rate or levy limit
D^{-5}	−0.005	0.026	−0.031	0.006
	(0.043)	(0.055)	(0.050)	(0.059)
D^{-4}	0.029	0.016	−0.017	−0.002
	(0.037)	(0.046)	(0.044)	(0.051)
D^{-3}	0.009	−0.002	−0.033	−0.019
	(0.059)	(0.060)	(0.060)	(0.061)
D^{-2}	0.091***	0.050	0.016	0.058*
	(0.029)	(0.036)	(0.048)	(0.034)
D^{0}	0.001	0.003	0.020	−0.011
	(0.021)	(0.025)	(0.025)	(0.025)
D^{1}	−0.032	−0.070**	−0.056**	−0.066**
	(0.020)	(0.031)	(0.024)	(0.031)
D^{2}	−0.071**	−0.113***	−0.102***	−0.122***
	(0.032)	(0.041)	(0.031)	(0.046)
D^{3}	−0.048*	-0.069	0.009	-0.091*
	(0.026)	(0.043)	(0.028)	(0.049)
D^{4}	−0.072*	−0.097*	-0.007	−0.112*
	(0.038)	(0.056)	(0.040)	(0.060)
# of observations	6,109	2,593	1,373	7,276

Note: Omitted period -1 is calendar year 2005. All models include year and county fixed effects as well as state specific time trend. All models include year and county fixed effects as well as state and county level covariates (including log of own source revenue minus property tax, share of intergovernmental transfer out of total revenue, county unemployment rate, log of county median household income, annual growth in housing price, share of senior female and black population, growth rate of state Gross State Product, log of state Medicaid eligibility income threshold and share of each industry to capture state economic structure.) Bootstrapped standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 10. Difference in differences estimates with different control groups (ETR)

DV: Effective property tax rate	*Various comparison groups*		
	(1) 6 States	(2) Keep mandate (IA, NJ)	(3) Tax rate limit (IA,KY)
ρ_1	-0.644^{*}	-0.827^{**}	-0.819^{***}
	(0.333)	(0.403)	(0.234)
N	4,076	1,375	1,918
	(4) No tax rate limit nor mandate (TN, MD, GA,VA)	(5) No effective tax limit (GA, IA, MD)	(6) No restrictive tax rate limits (No KY)
ρ_1	-0.733^{*}	-0.750^{***}	-0.644^{*}
	(0.445)	(0.227)	(0.389)
N	2,604	2,223	3,315

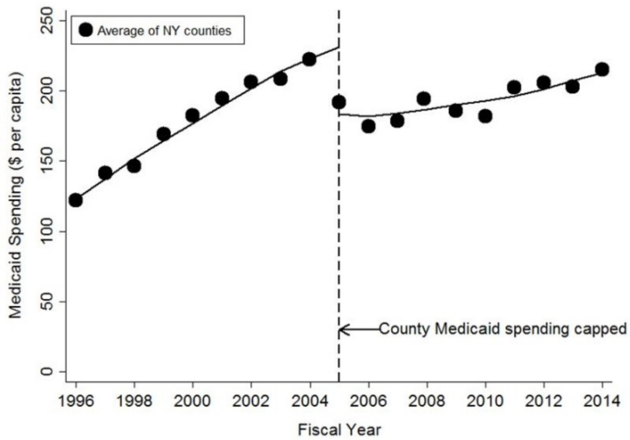
Note: All models include year and county fixed effects as well as state and county level covariates (including log of own source revenue minus property tax, share of intergovernmental transfer out of total revenue, county unemployment rate, log of county median household income, share of senior female and black population, growth rate of state Gross State Product, log of state Medicaid eligibility income threshold and share of each industry to capture state economic structure.) Bootstrapped standard errors with 1,000 replications reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 11. Property tax relief estimates with various comparison groups: Event study

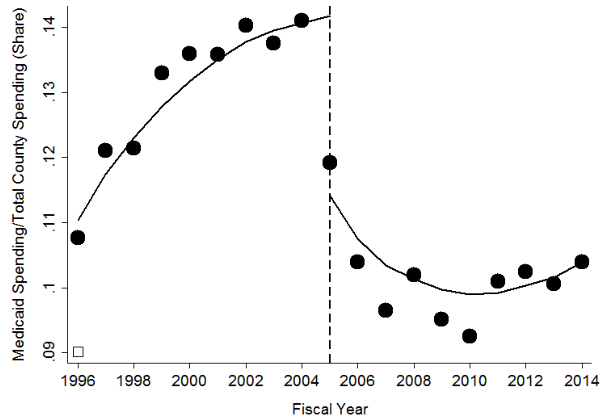
CONTROL GROUPS	(1) 6 States	(2) Keep mandate (IA, NJ)	(3) Tax rate limit (IA, KY)	(4) No limits nor mandate (TN, MD, GA,VA)	(5) No effective tax limit (GA, IA, MD)	(6) No restrictive tax rate limits (No KY)
D^{-5}	−0.313	−0.014	0.498	−0.398	−0.422	−0.311
	(0.766)	(0.722)	(0.637)	(0.618)	(0.730)	(0.718)
D^{-4}	0.552	0.671	0.575	0.570	0.520	0.567
	(0.570)	(0.650)	(0.602)	(0.557)	(0.541)	(0.643)
D^{-3}	0.180	0.103	0.032	0.282	0.317	0.182
	(0.644)	(0.645)	(0.709)	(0.728)	(0.679)	(0.747)
D^{-2}	0.531	0.233	0.367	0.569	0.673	0.534
	(0.724)	(0.833)	(0.760)	(0.761)	(0.642)	(0.833)
D^{0}	0.250	−0.159	0.131	0.238	−0.045	0.232
	(0.760)	(0.793)	(0.721)	(0.816)	(0.627)	(0.671)
D^{1}	1.239	0.663	0.701	1.289	0.980	1.231
	(0.863)	(0.768)	(0.866)	(0.876)	(0.931)	(1.080)
D^{2}	−1.409***	−2.042***	−2.127***	−1.046*	−1.303**	−1.423**
	(0.490)	(0.539)	(0.503)	(0.584)	(0.543)	(0.632)
D^{3}	−1.087*	−2.240***	−1.941***	−0.791	−1.559***	−1.097*
	(0.592)	(0.556)	(0.607)	(0.481)	(0.534)	(0.590)
D^{4}	−1.064*	−1.992***	−1.920***	−0.867	−1.513***	−1.067*
	(0.546)	(0.607)	(0.615)	(0.569)	(0.563)	(0.583)
Constant	1.948***	−4.403**	−9.731*	3.146**	−1.941*	1.792***
	(0.656)	(1.876)	(5.831)	(1.386)	(1.125)	(0.524)
N	4,076	1,375	1,918	2,604	2,223	3,315

Note: All estimates are from an event-study model, using equation (1) and are coefficient estimates of variable $D^{p(t)}$ that measure the differential change in outcomes between the treated counties and comparison counties in each $p(t)$, relative to period -1. Omitted period -1 is calendar year 2005. All models include year and county fixed effects as well as state and county level covariates. $\bar{Y}^{-1}$ of treated counties in New York State is equal to 7.856. Bootstrapped standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

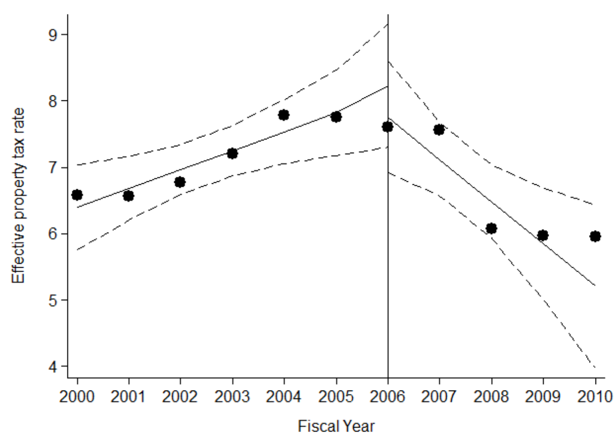
Figure 1. Stylized Facts: Time trend in Medicaid spending and property tax rate



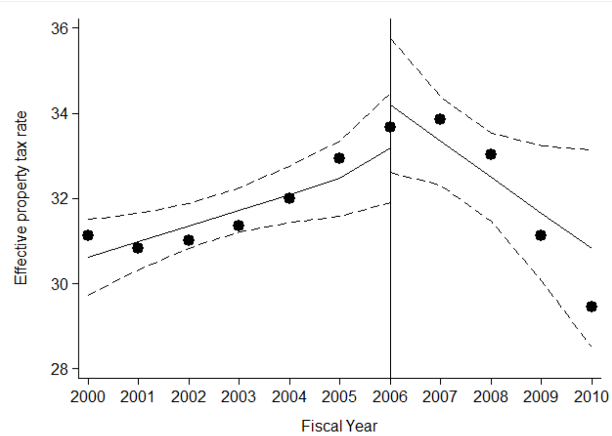
Panel A. Medicaid spending per capita (level)



Panel B. Medicaid / Total spending (share)



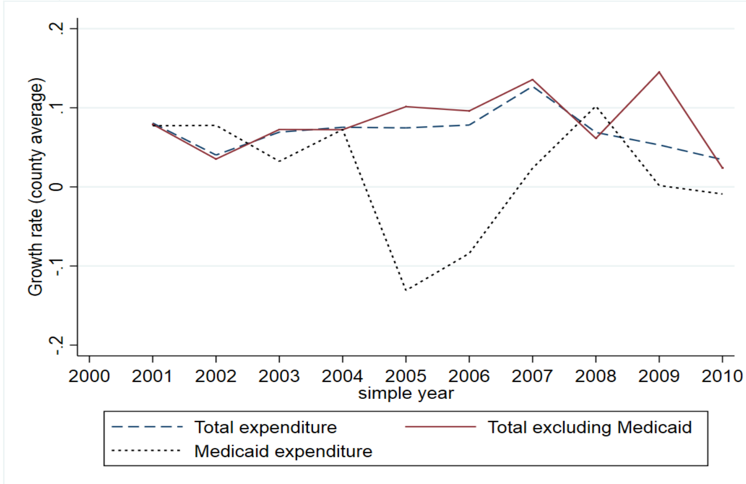
Panel C. County only ETR



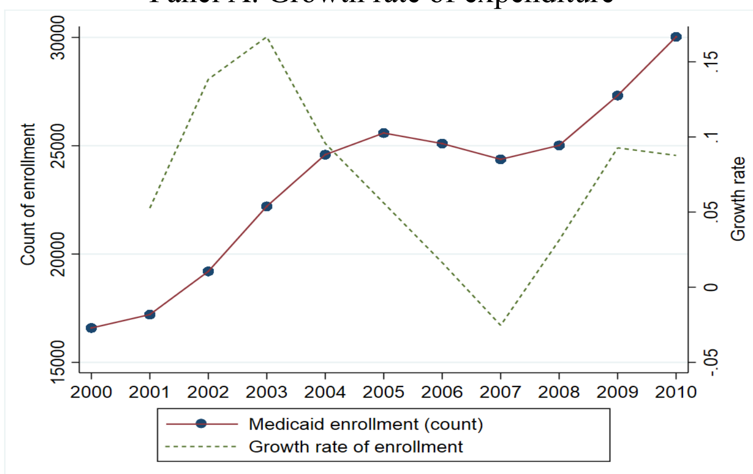
Panel D. Total countywide property ETR*

Note: Each dot depicts the simple average value of the dependent variables among all 57 counties in New York State. Panel C illustrates the county specific tax rate while Panel D shows the countywide rate that includes the overlapping local rates levied by other local governments within the county area. *Source:* New York Office of Real Property Tax Service, New York State Comptroller's Office

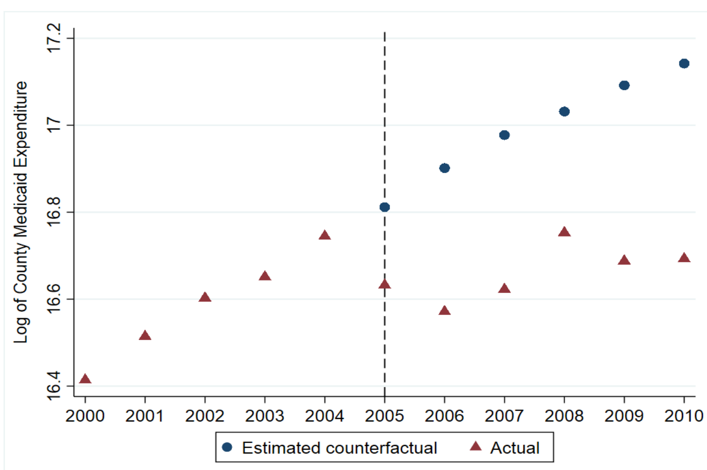
Figure 2. Total and Medicaid expenditure: Time trend



Panel A. Growth rate of expenditure



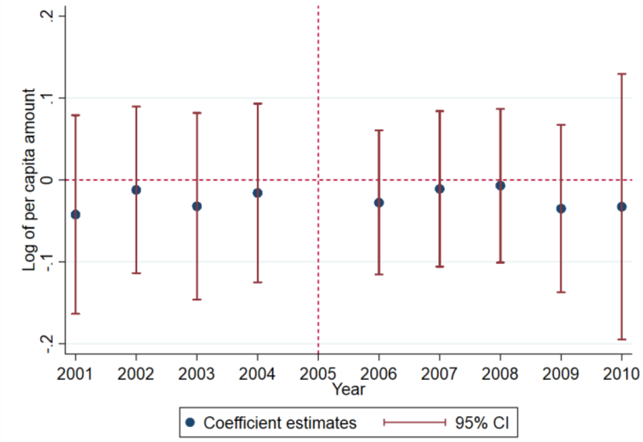
Panel B. Count and growth rate of Medicaid enrollment



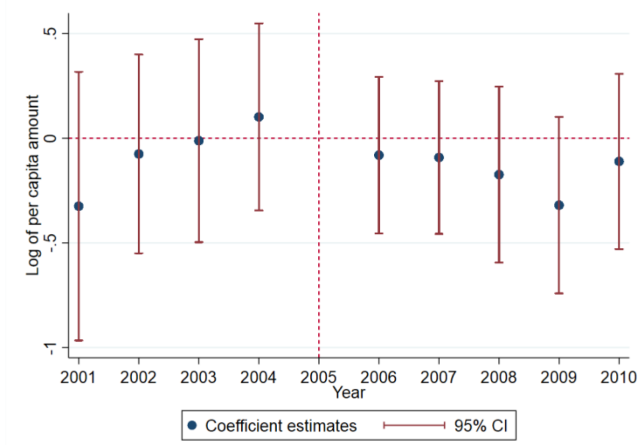
Panel C. Actual county level Medicaid expenditure and linear projection

Source: New York State Open Book, New York State Comptroller's Office *Comprehensive Annual Financial Report* and New York Department of Health

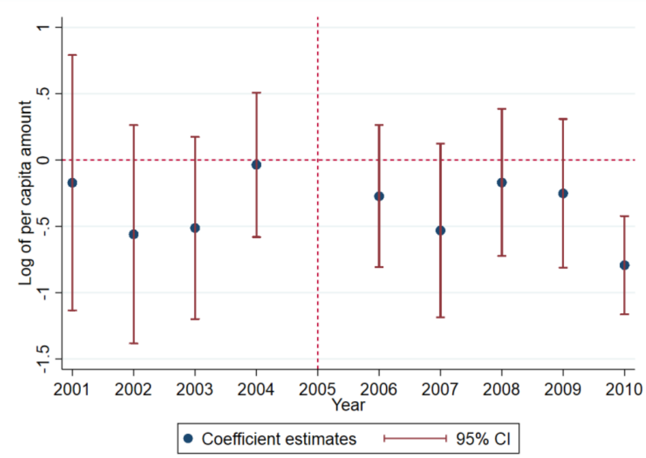
Figure 3. Budgetary spill-over effect: Event study estimates (level, per capita amount)



Panel A. Total spending excluding Medicaid

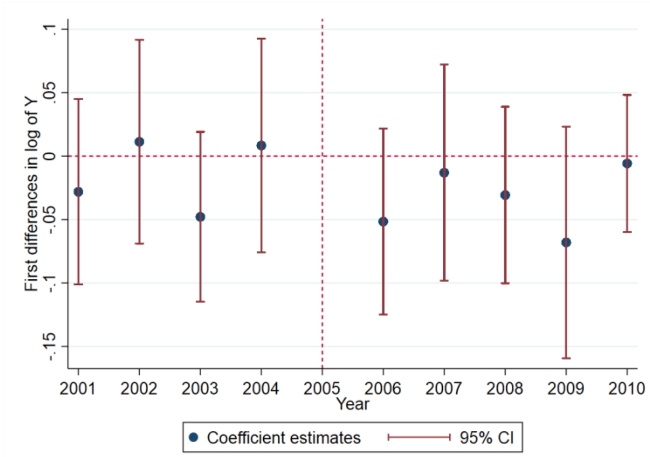


Panel B. Debt

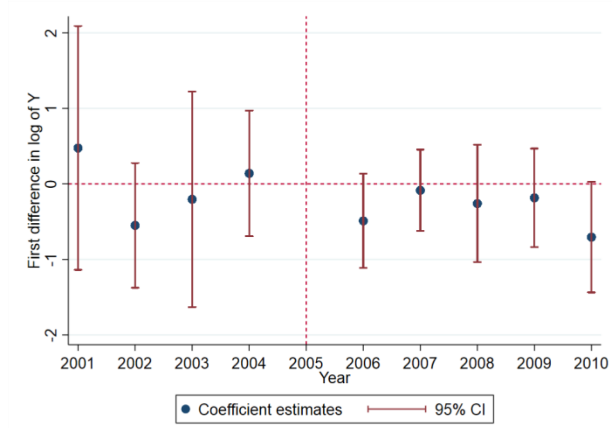


Panel C. Unreserved fund balance

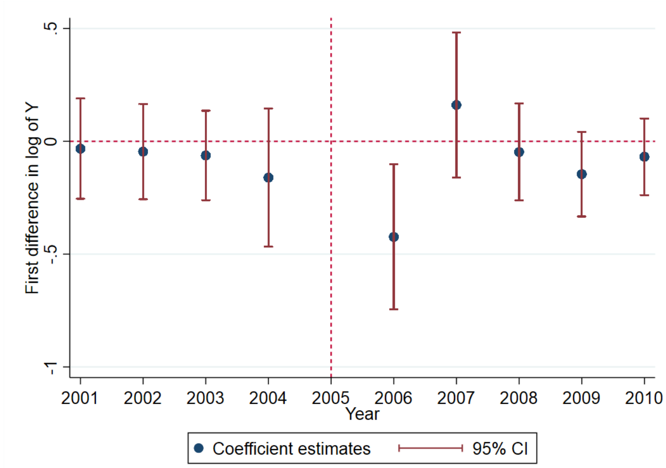
Figure 4. Budgetary spill-over effect: Event study estimates (growth rate)



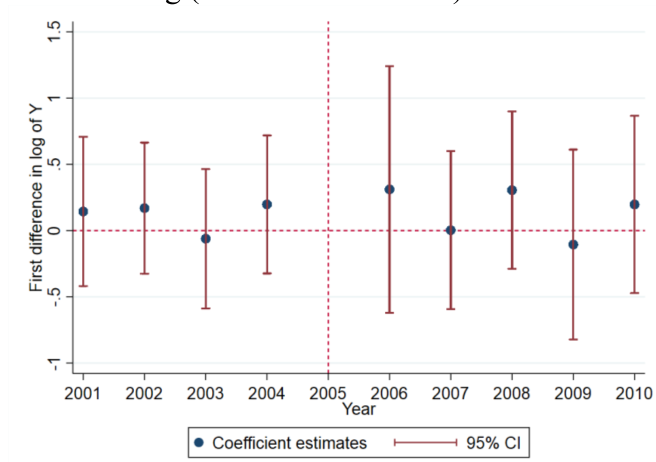
Panel A. Δlog (Total spending excluding Medicaid)



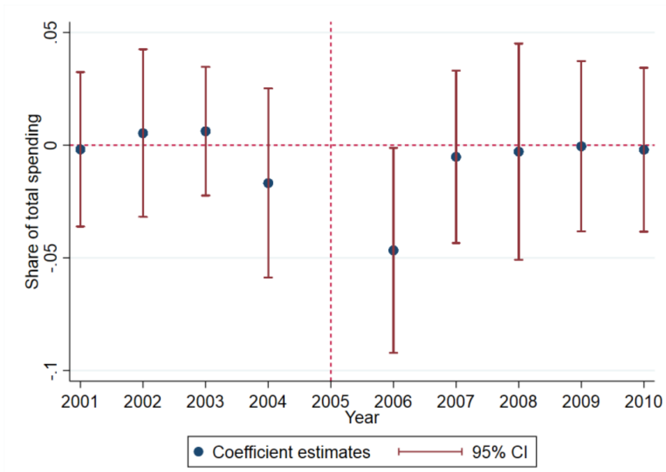
Panel B. Δlog (Unreserved balance)



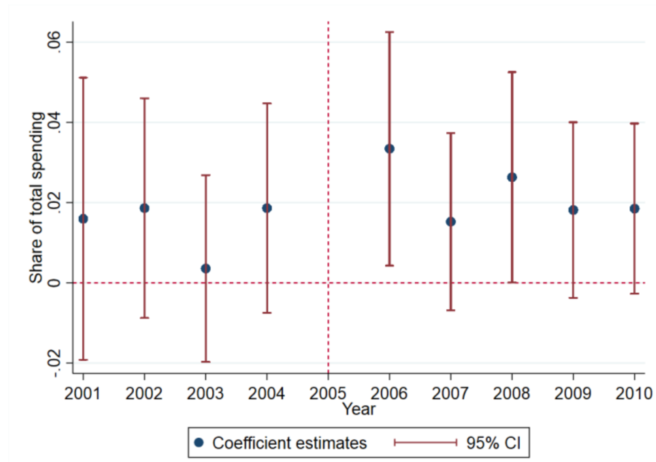
Panel C. Δlog (General government expenditure)



Panel D. Δlog (Other)

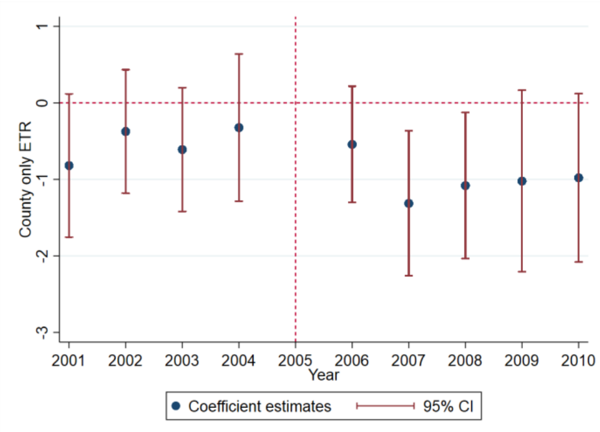


Panel E. General government (share of total budget)

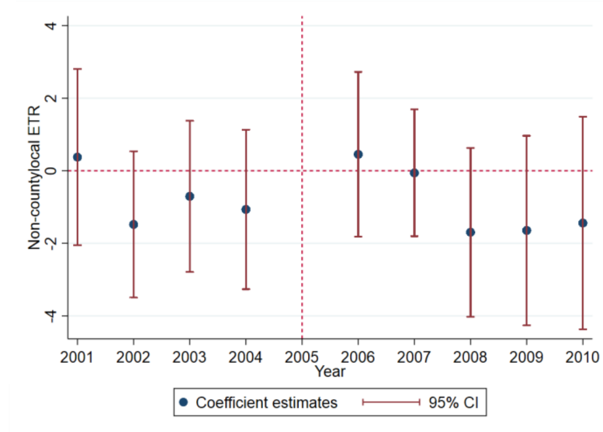


Panel F. Other (share of total budget)

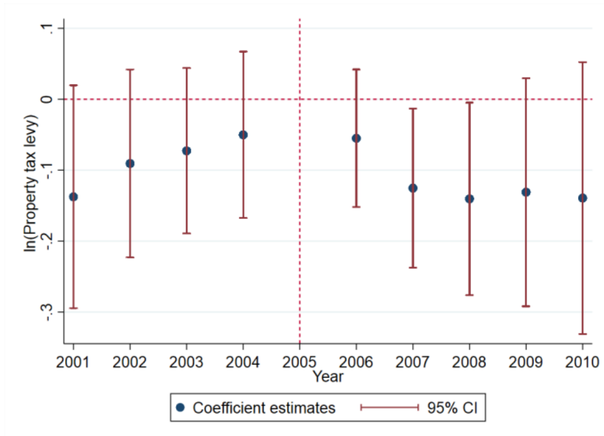
Figure 5. Impact of Medicaid cap on property tax levy and rate: Event study estimates



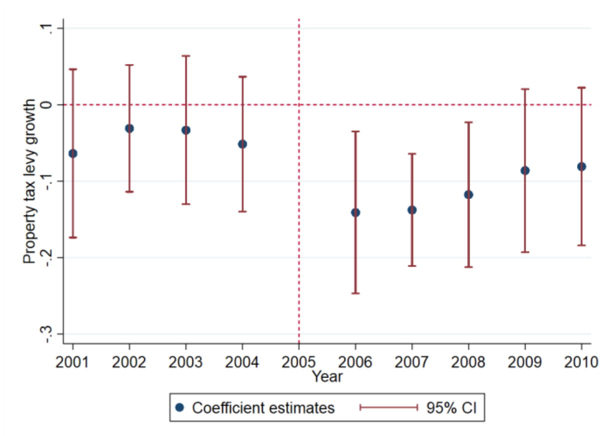
Panel A. County only ETR



Panel B. Non-county local ETR



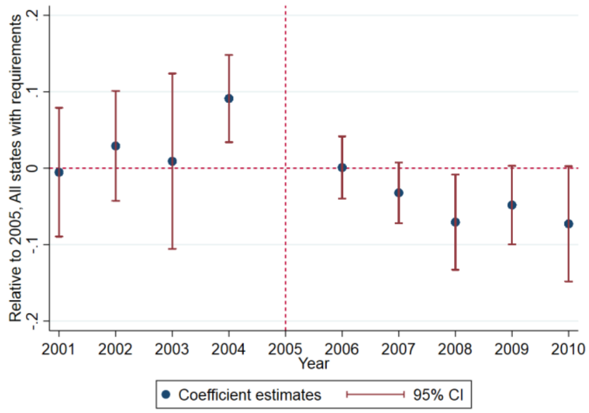
Panel C. Log of property tax levy



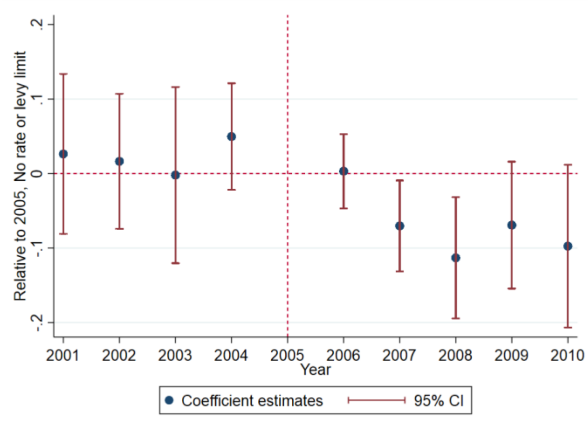
Panel D. Property tax levy growth

Note: The event study estimates in Panel A compares effective tax rates of 45 NY treated counties relative to 12 unaffected NY counties that did not experience a negative decline in Medicaid growth rate between 2005 and 2006. Estimates in Panel B uses the combined ETRs of overlapping local jurisdictions that levy tax(excluding county government) within a county area as the dependent variable. Estimates in Panel C and Panel D are each from models using log of property tax levy amount (in 2010 constant dollars) and first difference in log of property tax levy as the dependent variable. *Source:* New York Office of Real Property Tax Service

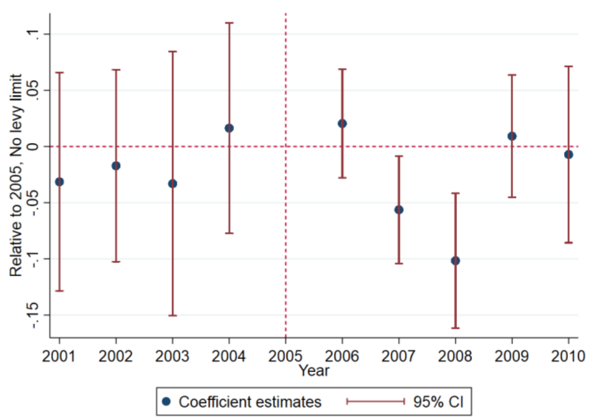
Figure 6. Impact of Medicaid cap on property tax levy growth: Event study estimates



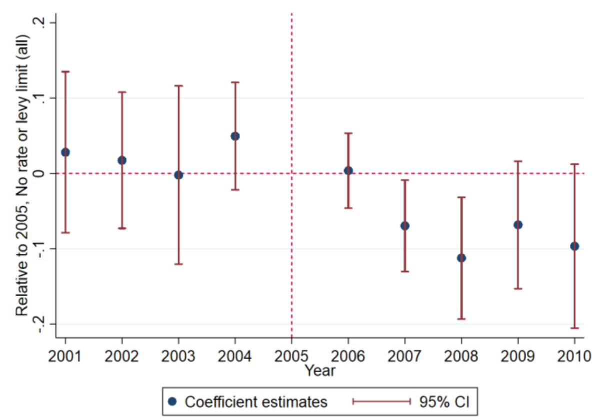
Panel A. States with Medicaid mandate



Panel B. States with mandate, no levy or rate limit



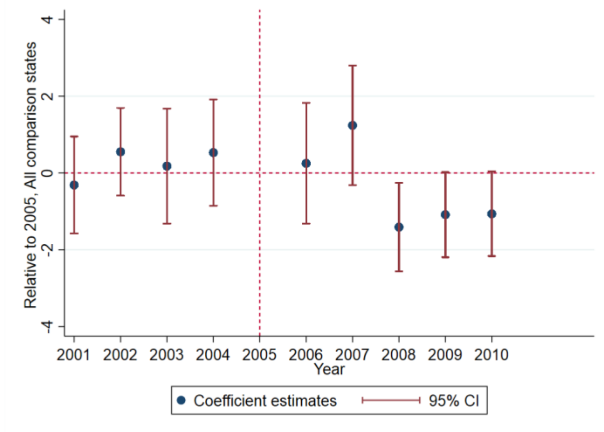
Panel C. States with mandate but no levy limit



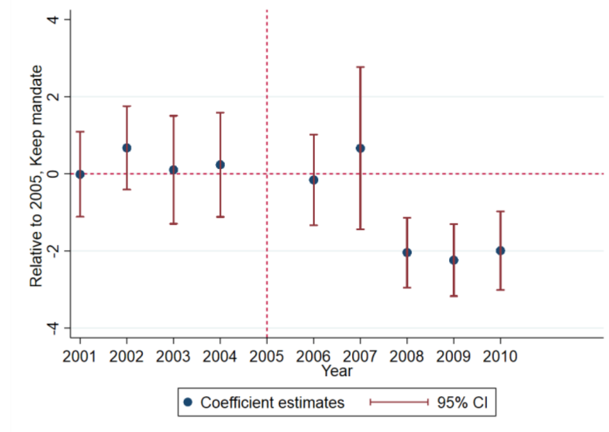
Panel D. No levy or rate limit (regardless of Medicaid mandate)

Note: The event study estimates in Panel A compares 45 NY treated counties relative to 1,309 counties in 21 control states. Panel B uses 344 counties in 8 states, while Panel C uses 155 counties in 5 states. All comparison units are in states that required their localities to pay for Medicaid, with the exception of subgroup used in Panel D. Estimates in Panel D are from a subsample using 1,117 counties in all 19 control states that did not have levy or rate limit, regardless of the presence of local Medicaid mandate.

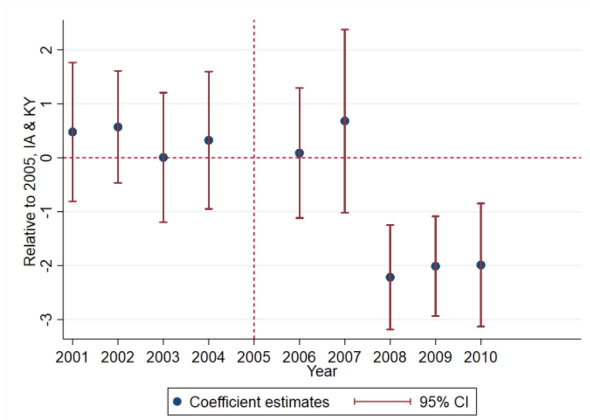
Figure 7. Event-study estimates: NY versus various comparison counties in other states



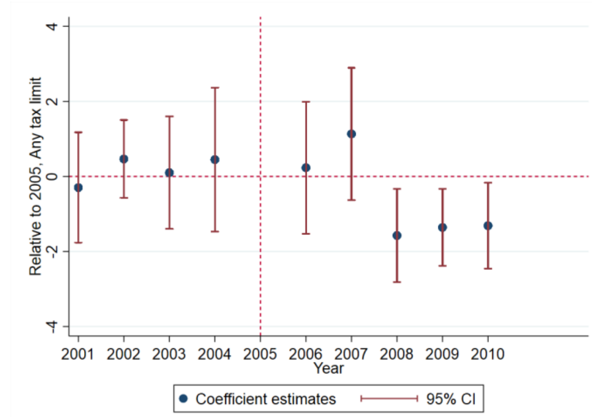
Panel A. All six comparison states



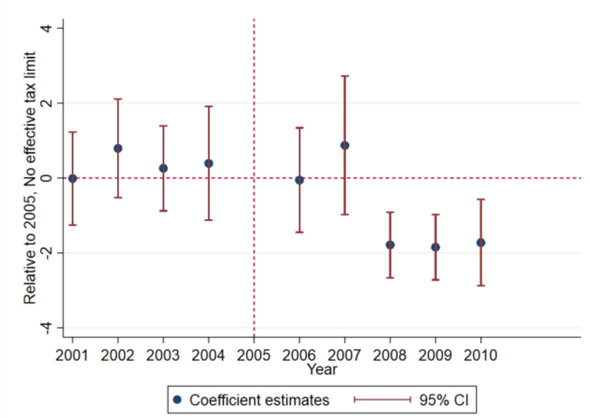
Panel B. Keep Medicaid mandate (IA, NJ)



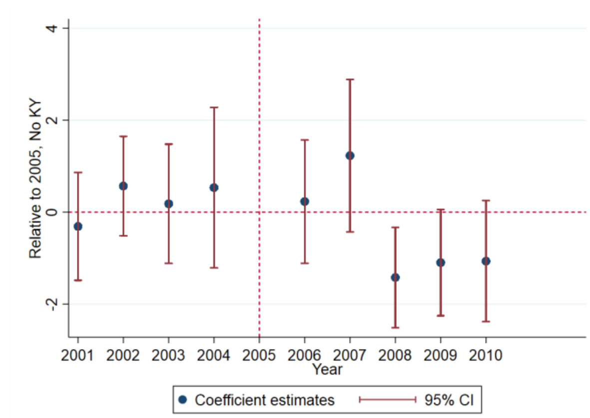
Panel C. Any tax rate limits (IA, KY)



Panel D. Any tax limits (IA, KY, VA)



Panel E. No effective tax rate limit (IA, MD)



Panel F. No restrictive tax rate limit (No KY)

Source: New York Office of Real Property Tax Services

Appendix

Table A1. Robustness checks: DD estimates with alternative specifications

DV: Δln (Property tax Levy)	States with Medicaid mandate			All states	
	(A) All states with mandate	(B) No rate or levy limit	(C) No levy limit	(D) No rate or levy limit	(E) All states with mandate
	Inverse probability weighting (clustered)				
ρ_1	−0.008	−0.062**	−0.161***	−0.069***	−0.101*
	(0.034)	(0.031)	(0.055)	(0.027)	(0.055)
	Propensity score as control variable (bootstrap)				
ρ_1	−0.017	−0.062***	−0.111***	−0.066***	−0.099***
	(0.023)	(0.019)	(0.034)	(0.017)	(0.029)
Y^{-1}	0.119	0.073	0.061	0.065	0.061
N	6,109	2,593	1,373	7,276	3,794

Note: Treated New York counties' mean value of outcome variable is 0.107.
*** p<0.01, ** p<0.05, * p<0.1

Chapter 2.

Estimating Returns to Scale in Property Assessment: Coordination among Assessing Units in New York

Yusun Kim

Department of Public Administration

Syracuse University, Syracuse, New York, 13244-1020

ykim79@syr.edu

John Yinger

Department of Public Administration and Economics

Syracuse University, Syracuse, New York, 13244-1020

jyinger@maxwell.syr.edu

Yilin Hou

Department of Public Administration

Syracuse University, Syracuse, New York, 13244-1020

yihou@maxwell.syr.edu

Abstract

In this paper, we test whether there are diseconomies or economies of scale in property assessment. We examine the cost effects of an alternative policy option for small tax assessing jurisdictions other than consolidation. We focus on tax assessing jurisdictions in New York that that formed coordinated units that unifies assessing functions, while maintaining their individual authority to levy property taxes. We test whether such decisions lead to cost savings and estimate the returns to scale in property assessment, using the cost function approach. We address the potential selection bias of forming coordinated units with multiple instruments, including the spatial intersection of borders and the history of inter-municipal cooperation in providing public services among neighboring jurisdictions. We use administrative data on towns and cities in New York State from 2003 to 2014. We find evidence that coordination increases adjustment costs in the short run among smaller units and leads to economies of scale among relatively larger units. This study potentially contributes to the literature on returns to scale in public service provision and to the practice of property tax administration.

JEL codes: H2, H7, R51

Key words: Property tax, Assessment, Local financial administration, Economies of scale, Cost function

1. Introduction

In this paper, we aim to estimate the returns to scale in property assessment. The property tax is a quintessential source of local revenue and autonomy in the United States. The administration of property tax, mainly assessment and collection, is typically decentralized to the county or municipality level. One of the major challenges in administering property tax is estimating the tax base in a fair and accurate manner, which is the task of the Tax assessors' office of each assessing jurisdiction. There are two main features of decentralized assessment: The (1) lower the *level* of government that conducts assessment, (2) the smaller the *size* of the assessing jurisdiction becomes. Property assessment is closely tied to the levy function, both functions which reflect fiscal autonomy of local governments. Decentralized property assessment may also be conducive to voters' preference revelation that can lead to optimal provision of public goods and services (Oates, 1991). In this line of thinking, proponents of decentralization often argue that lower level of governments may be more accountable to local residents' demand for public services.

However, there are apparent tradeoffs for greater accountability and local autonomy. Smaller assessing units often are challenged by lack of own-source revenues and human resources, which leads to suboptimal assessment practices. Property assessment requires a certain level of professionalism, specialized staffs and capital resources to maintain accurate valuation of properties. Therefore, there may be potential cost benefits from economies of size as long as assessment conducted by each assessor, does not diminish in quality as the number of parcels one overlooks increases.

There is little evidence on returns to scale in property assessment, but substnatial work on other public services. The literature on returns to scale in education (Tholkes, 1991; Pratten, 1991; Duncombe & Yinger, 2007) and police or fire services (Wasylenko, 1977; Duncombe & Yinger, 1993) provide abundant evidence on the cost benefits from economies of scale. On the other hand, other studies have challenged such view through estimation from production functions, suggesting that diseconomies of size may arise from lower monitoring pressure and staff motivation as well as higher transportation or adjustment costs (Kenny, 1982; Cotton, 1996; Howley, 1996; Duncombe & Yinger, 2007). Whether there are economies or diseconomies of scale in property assessment is an empirical question that deserves more attention. Only a couple of papers have posed this question, providing only limited descriptive results (Wicks et al., 1967; Sjoquist & Walker, 1999).

In this paper, we use data from NYS to assess whether merging the property assessing function leads to cost savings. Many states including New York have provided financial incentives to encourage consolidation of local tax assessing units or centralization of the assessment function to counties, under the assumption that this will provide benefits of economies of scale. We focus on a certain type of inter-municipal cooperation or reorganization of the unit of public service provision, which can be less costly than consolidation. The case we study is the formation of a coordinated assessing unit in New York where two or more local assessing governments that agree to combine their office of tax assessors, but preserving other local autonomy in budgeting and levying taxes.

By testing the presence and estimating magnitude of returns to scale using a unique panel data set of NY assessing jurisdictions from 2003 to 2014, this paper attempts to estimate an optimal jurisdiction size for efficient property tax assessment. Although we use a sample of tax assessing jurisdictions in NY, the population of this study is all local governments that play the role of tax assessment in a decentralized property tax system. On a broader scale, this paper extends the perennial discussion on equity and efficiency in public service provision to the study of property tax administration.

2. Literature Review

The existing literature on returns to scale in the provision of public services provides mixed evidence of economies and diseconomies of scale. The earlier studies of economies of scale on fire, roads, police services and public libraries have relied on *ad hoc* functional forms rather than reflecting on economic theories to model costs (Ahlbrandt, 1973; Walzer, 1972; Deller et al., 1988). Later research started adopting Bradford et al (1969) pro duction cost function framework, which adapted the economic theory of cost minimization at firm level to the public sector (Fox, 1981). Duncombe & Yinger (1993) introduced compelling estimates in fire protection services and provided formal definition of various dimensions in returns to scale. A number of empirical studies in the education finance literature employed the cost function framework and reported evidence of economies of size, the results of which vary by different types of spending (Ratcliffe et al., 1990; Downes & Pogue, 1994; Duncombe et al. 1995) or finds an optimal enrollment size that minimizes cost such as Duncombe et al (1996) or Imazeki & Reschovsky (2004). Tholkes (1991) and Pratten (1991) summarized the potential mechanism of economies of size in education, which also applies to other relevant public service provision: Such mechanisms include indivisibilities of labor input, increased dimension conducive for sharing capital or technology, specialization, price benefits of scale in purchasing inputs, lower cost of innovation and positive learning spillover effects. On the other hand, another strand of literature using production function to study the effects of school size on student performance provided counterevidence against such findings (Deller & Rudnicki, 1993; Walberg & Fowler, 1987; Ferguson, 1991; Lee & Smith, 1977).

Andrews et al (2002) provide a detailed review of the empirical literature estimating returns to scale in education, underlining the methodological limitations across studies such as the measurement issue of performance, efficiency and outcome variables. More importantly, they outline the importance of addressing simultaneity bias and omitted variable bias in cost function models often found in studies that fail to include important determinants of cost or treating potentially endogenous variables as exogenous. While most studies relied on cross-sectional variation in size, Duncombe and Yinger (2007) is one of the very few studies that studied the cost effect of school district consolidation by employing dramatic variation in size of the service provider unit over time to estimate the effect on cost.

Despite the long history of the property tax in the United States, the academic literature is very thin on the returns to scale in property assessment. Among the earlier studies on the cost of property tax administration, Netzer (1966) and Wicks and Killworth (1967) were the first study to provide jurisdiction specific estimates of property tax administration costs which is 1.5 percent of property tax revenue, on average. As of now, there is only one empirical study that examined the relation between the size of assessment jurisdiction and assessment costs. Sjoquist and Walker (1999) used a sample of 138 county-level assessment offices in Georgia to examine economies of scale in assessment cost. Using a translog cost function model, they find evidence of significant economies of scale, and estimate the assessment volume elasticity of total cost to be 0.3. They suggest that hypothetical consolidation of smaller assessing units would reduce total cost by approximately 20%. However, there is no discussion about the potential endogeneity in performance measure or consolidation decision, which leads to a biased estimate of the elasticity.

Furthermore, their estimates of consolidation's effect on cost is based on hypothetical consolidation and grounded in strong assumptions. Therefore in this paper, we intend to contribute to the literature on estimating returns to scale by incrementing potential endogenous variables and more importantly, by employing a sample of assessing jurisdictions in New York that combined assessing functions with neighboring jurisdictions.

3. Property Assessment in the State of New York

As a strong home rule state, New York State has a highly decentralized property tax system. Even though the number of assessing units has declined since 1983 when there used to be 1,546 assessing units, there are still 994 local governments including mostly towns (932) and cities (62) that conduct property assessment. Variations in assessment practices across the assessing jurisdictions in NY is large. Many of the 994 assessing units are small and rural jurisdictions that are challenged to conduct reappraisal on a regular basis. In addition to the size and environmental characteristics of assessing jurisdictions, there is a wide variation in the individual level traits of assessors as well as other institutional features of the assessment system (such as assessment cycle or ratio) in NY.

There are 524 jurisdictions where 180 assessors serve for multiple jurisdictions instead of having a sole-assessor per jurisdiction (ORPTS, 2011). By 2017, in more than 94% assessing jurisdictions, assessors are appointed by the municipal board, while less than 6% of assessing jurisdictions electing assessors (ORPTS, 2017)[33]. The increasing trend of appointing assessors

[33] A few number of jurisdictions that elect assessors either maintain boards of three elected assessors or elect a sole assessor. The length of each term for appointed or sole elected assessors is six years, while the three elected assessor members of a board serve for four years.

reflect the movement toward professionalism since appointed assessors are better insulated from political pressure than elected assessors.

Assessing jurisdictions in NY are also not bound by any legal requirement of uniform cycle or ratio of assessment. Each assessing jurisdiction has its own tax calendar and inconsistent level or cycle of reassessment across time. Instead of mandating a specific ratio or cycle, the State government of New York provides three different types of financial aid programs to assist property assessment functions at the local level. First, instead of mandating a specific reassessment cycle, New York provides Cyclical Reassessment Aid as an incentive for assessing jurisdictions to conduct reappraisal more frequently. Second, the state government provides aid through Consolidated, Coordinated and County Assessment Program. To encourage reduction in the number of small assessing jurisdictions, the NY state government introduced a Coordination Assessment Program (CAP) in 1994 as an intermediary alternative for consolidation, since consolidating two or more local governments is a long process and may incur high political and adjustment costs.

All CAP agreements requires a majority vote of the Town or City Board in order to be implemented. The main agreements of a CAP include employing a single assessor, assessing at a uniform percentage of market value, using the same assessment calendar and preparing a single assessment roll among participating jurisdictions. Total cost for assessment would initially be incurred by a participating jurisdiction that is the employer of a sole assessor, but then shared by other participants on a pro-rata basis of the total number of parcels within each jurisdiction. All parcels in participating jurisdictions within the same CAP will be assessed at the same uniform percent of the full market value. The state government also reports identical equalization rates for jurisdictions in the same CAP. On the other hand, each participating jurisdiction maintains their assessment appeal proceedings separately and can individually file complaints against the state's calculation of equalization rate. Furthermore, while the jurisdictions share assessment function, they impose different tax rates for each local government unit in a county or a school district.

CAP participants receive a one-time lump sum payment of state aid in the year they unify their assessing functions with their neighbors. The maximum amount of state aid payed to each municipality is capped at $140,000 (which used to be maximum $7 per parcel since 1996) until 2005. Since 2006, the payment was limited to $100,000 per municipality. Between 2000 and 2010, 101 jurisdictions have newly participated in 51 CAPs while 17 units have dissolved. We utilize such variation in CAP activity which can be translated into variation in size of assessing units, in order to estimate the returns to scale in our analysis.

4. Analytical Framework and Models

4.1. Cost Function Framework

We follow the standard cost function approach for estimating returns to scale in public production developed by Bradford et al (1969). The cost function approach relies on the economic theory of cost minimization for the provision of public goods and services, and uses a modified version of the standard private sector cost function at the firm level. We adopt the framework commonly used in the Education finance literature for studying the cost effect of school district consolidation, and adapt it to the context of property tax assessment (Duncombe & Yinger, 1993; 2007; 2011; Duncombe et al., 1995; Downes & Pogue, 1994; Imazeki & Reschovsky, 2004).

The process of providing property assessment service might be relatively simpler than the provision of other public services, because other services involves different types of resources and multiple actors with vested interests. In a cost model for public production process for local assessing jurisdictions. The tax assessing local governments employ various inputs (denoted by the vector $\boldsymbol{I_i}$) such as personnel cost for hiring a full-time assessor and staffs in the assessing office or contractual expenses should they hire external contractors to conduct field visits for property assessment. The various inputs will be translated into intermediate output of property assessment service, $G_i = g(I_i)$, such as revaluation of property values or updating tax maps and conducting a sales ratio study. G_i also reflects the production technology that translates factor inputs to actual activity of property assessment. Nevertheless, largely due to the limitation of data on detailed inputs and intermediate outputs, we instead use performance measures of property assessment service such as the coefficient of dispersion that ultimately matter to the voters as the final output, S_i. The key mission of property assessment is to minimize the variation in assessment ratio across home owners. The performance of property assessment, if measured by the coefficient of dispersion (COD) that reflects assessment uniformity, is also a function of the intermediate output (G_i); physical factors such as total number of parcels (N_i) that needs to be assessed as well as other neighborhood and jurisdiction level environment variables including population density (E_i). The performance model can be written as the following: $S_i = h(N_i, G_i, E_i)$ or $h(N_i, g(I_i), E_i)$.

The associated cost function that models the minimum cost for property assessment conditional on best technology available, can be written as $C_i = c(g(I_i), W_i)$; where cost is a function of intermediate output and input prices (W_i). The conventional approach is to use actual expenditure as a proxy of $\mathrm{C_i}$, the underlying assumption of which is constant dollar per unit of public service produced. Cost C_i may be equal to observed total expenditure (EX_i) for property assessment, assuming no technical inefficiency within the assessing unit. Solving the performance model for G, and substituting into cost function equation, while replacing C with actual operating expenditure of the assessment office in jurisdition *i*, EX_i, we get the following cost function form:

$$EX_i = c[h^{-1}(N_i, S_i, E_i), W_i] = N_i^{\beta} S_i{}^{\tau} E_i{}^{\gamma} W_i{}^{\lambda} \qquad (1)$$

where W_i indicates personnel cost in assessor's office, as the price of major factor input. We assume that the price of capital input is the same across assessing jurisdictions within NY State, following Sjoquist & Walker(1999). Based on an additional assumption about unity elasticity of all inputs, the Cobb Douglas cost function equation (2) can be transformed to a flexible logged form that is estimable using linear regression methods and panel data as the following:

$$ln(EX_{it}/N_{it}) = \alpha + \beta\, ln(N)_{it} + \tau\, ln(S)_{it} + \gamma\, ln(E)_{it} + \lambda\, ln(W)_{it} + \varepsilon_{it}. \qquad (2)$$

Among the three dimensions in public service provision (i.e. returns to S, G and N), we focus on estimating the change in assessment expenditure with changes in jurisdiction size, measured by the total number of parcels. Using parameters from equation (2) and its variants, we can estimate economies/diseconomies of size in assessment. The elasticity between property assessment expenditure per parcel (EX_{it}/N_{it}) and jurisdiction size (N_{it}), $\frac{\partial \mathrm{EX/N}}{\partial \mathrm{N}}$ can be estimated by β from equation (2). If assessment cost per parcel declines by increasing total number of parcels, we suggest there is economies of size in property assessment, controlling for assessment uniformity and other socio-economic cost related factors. While the elasticity estimate consists of technical economies of scale and congestion, we do not need to separate the two for our main purpose. Unlike the case of fire protection where Duncombe & Yinger (1993) raise the issue of congestion, we assume that the environmental factors such as population density do not affect factor substitution in property assessment activity. In other words, we consider the potential effect of a unit increase in the jurisdiction size on the level of property reassessment (G_i) required to maintain a given level of assessment uniformity (S_i) to be independent of the environment variables (E_i). Our context is also in a single-product scenario; we do not separate different classes of properties as multi-product. This is a result of the data available, because we only observe total cost and input costs for assessment of all properties and not by class. In other words, we disregard economies of scope because we pool properties in all classes.

The estimable expenditure model in equation (2) allows flexibility to add interaction or nonlinear terms of the size variable. We add quadratic terms of the variable $\ln(\mathrm{N})_{\mathrm{it}}$ to test if (dis)economies of size diminish with size and identifying an inflection point that can be informative. The inflection point would indicate the minimum size of assessing unit where the total cost starts to decline instead of increase. Also, we include share of parcels that are exempted from assessment in the vector Z_{it} to partially capture inefficiency in property assessment function. This reflects reported complaints from town assessors about how the process of identifying exempt parcels take away the limited time and staff resource that needs to be spent on actual reappraisal. In interviews conducted as part of this study and a survey conducted through the New York Association of Assessors, assessors reported that administering large quantity and categories of property tax exemptions (reviewing renewals) is one of the major challenges for assessment (Bick, 2016) [34]. If there are any bureaucratic inefficiencies among assessing jurisdictions that cannot be directly observed, we assume they would not vary over time which should be absorbed by the jurisdiction fixed effect, δ. The baseline assessment expenditure model is as follows[35]:

$$\ln(EX_{it}/N_{it}) = \alpha + \beta_1 \, ln(N)_{it} + \beta_2[ln(N)_{it}]^2 + \tau \, ln(S)_{it} + \gamma \, ln(E)_{it} + \lambda \, ln(W)_{it} + \theta_t + \delta_i + \varepsilon_{it}. \qquad (3)$$

The literature underlines the importance of accounting for the potential simultaneity bias of the voters' demand for services affecting both the expenditure and outcome measures (Fox, 1981; Andrews et al., 2002). Instead of including output measures in the final expenditure model, Ratcliffe et al(1990) and Downes & Pogue (1994) indirectly model S_i by incorporating a behavioral model of demand for education services and substituting decisive median voter's demand equation into the cost function. However, one caveat of this approach is that the measurement error in modeled outcome measures may bias the elasticity estimates. Instead, we treat S_i as endogenous and employ exogenous determinants of demand for assessment uniformity as instruments in our final structural equation. Following Duncombe & Yinger (2007) approach which is an extension of Case et al's (1993) classic "copycat" theory, we assume that the median-voter demand for property assessment services among median voters within a given jurisdiction may partly be influenced by neighboring jurisdictions' assessment performances and assessment uniformity. We use median tax share and average residential COD among neighboring

[34] Robert Bick, Assessor, Town of Clay, New York, Personal interview, September 27, 2017; Robert Harris, Assessor, Flat Creek of Montgomery County, New York & William F. Roehr, Managing Principal, Montgomery County, New York, April 21, 2017.

[35] An alternative is following Duncombe & Yinger's approach (1998, 2001, 2007) where they model efficiency based on observable characteristics that may affect (in)efficiency via monitoring efforts among voters and local officials' incentives to assess more efficiently.

jurisdiction within the same county to instrument for S_i. We use the same theory to select instruments for assessor wage, W_i: The determinants of county level local labor market conditions such as wage in the manufacturing industry or relevant private sector as well as unemployment rates are used as instruments for assessor salary.

4.2. Empirical Model

Unlike previous cost function research that relies on cross sectional data, the main source of variation we exploit is changes in assessing units over time. Specifically, we observe variation in size when a jurisdiction enters a CAP and combines its assessing function with its neighbors CAP participants. In order to cope with potential endogeneity in their decisions to join a CAP, we use multiple instruments based on the intersection of geographic borders and the history of inter-municipal cooperation in providing public services in a standard IV model. The outcome equation and treatment equation can be summarized as follows:

$$Y_{it} = f_Y(P_{it}, \boldsymbol{V}_{it}, \boldsymbol{X}_{it}, \boldsymbol{U}_{\text{it}}, \varepsilon_{it}) \qquad (4)$$

$$P_{it} = f_P(\boldsymbol{Z}_{it}, \boldsymbol{X}_{\text{it}}, \boldsymbol{U}_{\text{it}}) \qquad (5)$$

where Y_i denotes the dependent variable which is $\ln(EX_{it}/N_{it})$; $\boldsymbol{V}_{it}$ is a vector of other endogenous determinants outlined in the cost function (S_{it}, W_{it}, A_{ict} and R_{ict}); $\boldsymbol{U}_{\text{it}}$ is a vector of time varying unobservable traits of jurisdiction i and $\varepsilon_{it}$is the error term from the expenditure outcome equation. P_{it} is a dummy of CAP decision that equals to 1 if a jurisdiction *i* enter a CAP or were already in a CAP during the sample period and 0 otherwise. $\boldsymbol{Z}_{it}$is a vector of instrument variables for the CAP decision which will be elaborated below. Our underlying assumption of independence can be expressed as (6) which implies the exclusion restriction condition (7):

$$Z_{it} \perp \boldsymbol{U}_{\text{it}}, \varepsilon_{it} \,|\boldsymbol{X}_{\text{it}} \qquad (6)$$

$$Z_{it} \perp Y(p)_{\text{it}} \,|\boldsymbol{X}_{\text{it}} \text{ for all p} \in supp(P_{it}) \qquad (7)$$

In this paper, we estimate the economies of size among jurisdictions that became larger assessing units after joining a CAP. Therefore, we construct a new measure of size that reflects the CAP decision defined as the following:

$$\begin{cases} NC_{ict} = (N_{ict} + \sum_{j \neq i}^{J} N_{jct}) \quad \text{if } P_{ict} = 1 \; and \; P_{jct} = 1 \\ NC_{ict} = N_{ict} \quad \text{if } P_{ict} = 0 \end{cases}$$

where jurisdiction *i* and jurisdiction *j* are neighboring towns/cities in county *c* that become CAP partners. T_{ic} is an indicator of CAP participation that is 1 for all years if a jurisdiction ever entered a CAP during the sample period. We assume that the size of neighboring jurisdiction *j* is exogenous to jurisdiction *i*. The size of a jurisdiction *i* participating in a CAP equals to the original N_{ict} before joining a CAP jurisdiction and only becomes the enlarged NC_{ict} after they enter a CAP. For jurisdictions that never entered a CAP during the sample period, NC_{ict} are coded as N_{ict} for all years. This adjustment in the measure of size should also be reflected in the measure of the dependent variable which is newly defined as follows:

$$\begin{cases} EXC_{ict} = (EX_{ict} + \sum_{j \neq i}^{J} EX_{jct}) \quad \text{if } P_{ict} = 1 \; and \; P_{jct} = 1 \\ EXC_{ict} = EX_{ict} \quad \text{if } P_{ict} = 0 \end{cases}$$

The final model to be estimated by 2SLS is an extension of equation (3), but replaces the size and cost per parcel variables with the adjusted measures defined as above. This model also includes additional time varying observable covariates that are expected to be correlated with both NC_{ict} and EXC_{ict} for a jurisdiction i in county c and year t. All covariates for CAP participants in years they are in a CAP (whose $P_{ict} = 1$) should also be adjusted accordingly. In other words, the values of random variables in vector X_{ict} should be converted to the average of all CAP participants' values, weighted by population for demographic variables and by number of parcels for property related variables. The final structural expenditure model is expressed as follows:

$$\ln(EXC_{ict}/NC_{ict}) = \alpha + \beta_1\, ln(NC)_{ict} + \beta_2[ln(NC)_{ict}]^2 + \beta_3 P_{ict} + \tau\, ln(S)_{ict} + \gamma\, ln(E)_{ict} + \lambda\, ln(W)_{ict} + \rho\, ln(A)_{ict} + \varphi\, ln(R)_{ict} + \Pi\mathbf{X}_{\mathrm{ict}} + \theta_t + \delta_{ic} + \varepsilon_{ict} \tag{8}$$

where A_{ict} denotes state aid received for property assessment (including the one-time, lump sum amount of state aid for joining a CAP); R_{ict} indicates years since last reassessment and $\boldsymbol{X}_{ict}$ represents a vector of exogenous characteristics of jurisdiction i. In order to isolate an unbiased estimate of the returns to size in assessment, it is important to control for other potential channels through which CAP may affect assessment expenditure. One of these alternative channels is state aid, since CAP participants receive financial award by the state government. The direction of potential omitted variable bias in our elasticity estimate by excluding A_{ict} is ambiguous: Although state aid is significantly and positively correlated with CAP by construct, state aid (that often comes along with technical assistance) may either contribute to cost savings or it can lead to an increase in assessment expenditure. Nonetheless, it is important to include state financial assistance for property assessment in the model. We also control for the confounding effects of changes in reassessment cycle, to account for the possibility that jurisdictions that enter a CAP may be able to save costs by conducting reappraisal more frequently with their partners.

Another alternative channel is through changes in the level of assessment, since jurisdictions in a CAP are required to assess at the uniform % of true value. If entering a CAP leads to increase in the level of assessment through conducting more frequent assessment, R_{ict} may partly capture this effect on cost. As a robustness check, we test to see if our elasticity estimate is sensitive to the inclusion of level of assessment variable measured by state equalization rates.

The covariates in vector $\boldsymbol{X}_{ict}$ includes share of exempt parcels; share of commercial parcels; share of industrial parcels, population density, and population growth. The extent to which administrative environment changes over time at the town and city level is limited, and we expect the jurisdiction fixed effect δ_{ic} will capture any adjustment costs not associated with NC_{ict} but correlated with P_{ict} and the dependent variable. In several specifications, we also include CAP unit specific linear time trend. We weight our estimates by the original parcel count of each jurisdiction.

Following the cost function literature we use instruments for A_{ict} and R_{ict} by relying on the copy cat or yardstick theory, as in Duncombe & Yinger (2007). We assume that districts are influenced by decisions of similar comparable jurisdiction's decisions (in the same labor market area) and treat the neighbor jurisdictions' characteristics as exogenous to actual assessment cost in jurisdiction i at year t. The final instruments for A_{ict} and R_{ict} are the average amount of reassessment state aid received and years since last reassessment among neighboring jurisdictions within the same county.

We use three instruments for P_{it} and NC_{ict} in our final empirical model. The first instrument is the number of CAPs formed in the same county, excluding jursidiction i. This reflects the exposure to an environment of collaboration among neighboring tax assessing jurisdictions. We hypothesize that the more number of CAPs formed in the same county, the greater the probability for a given jurisdiction to enter a CAP. Indeed we observe positive correlation between this first instrument and the predicted probability of joining a CAP as shown in Figure 1.

$$z_{1,\mathrm{ict}} = \sum_{k \neq i} P_{kct} \mid k, i \text{ in the same county, } c$$

The other two instruments reflect jurisdictions' exposure and opportunities for inter-municipal cooperation for providing public services other than tax assessment. We use administrative data on state aid provided to municipalities that committed to collaborating with neighboring local governments. The second instrument is the mean count of other jurisdictions k who had experience in sharing the role of providing public services with its neighbor jurisdictions in the same county, which can be expressed as follows:

$$z_{2,\mathrm{ict}} = \sum_{k \neq i} DSHARE_{kct} \mid k, i \text{ in the same county, } c$$

where$z_{2,\mathrm{ict}}$ denotes the second instrument and $DSHARE_{kct}$ is a dummy indicator for jurisdiction k in the county c that had any experience in sharing public service with other towns or cities in the same county and received state aid for such collaborative activities at year t. The underlying hypothesis is that there would be a positive relation between the second instrument and decision to join a CAP, implicitly grounded on the assumption of path dependency behavior.

The third instrument is inspired by the so-called Bartik instrument or "shift-share" instrument that utilizes the interaction between variation in inflow of immigrants at the national level with immigrant's past geographic distribution at the city level to identify a short run casual effect of migration on various outcomes. In this paper, we adopt the intuition of using variation at the higher level of government and interacting with jurisdiction level spatial variation that does not vary over time and is exogenous to a jurisdiction's spending decisions at time *t.* We construct the third instrument, $\mathrm{z}_{3,\mathrm{ict}}$, which is the frequency of county c's $DSHARE_{ct}$ with other county governments[36] ($\mathrm{COUNTYSHARE}_{\mathrm{ct}}$), multiplied by the relative size of neighboring jurisdictions whose borders are contiguous to that of a given jurisdiction *i*.

$$\mathrm{z}_{3,\mathrm{ict}} = \frac{\#\text{ of } k \text{ with contiguous borders with } i, \text{ in county } c}{\#\text{ of } k, \text{ in county } c} \cdot \mathrm{COUNTYSHARE}_{\mathrm{ct}}$$

We also instrument for the quadratic size variable, $[ln(NC)_{ict}]^2$, following Wooldridge (2000)[37]. The first stage model can be expressed as the following equation (9) or alternatively, use a multi-year average (three recent years until year *t*) values of the three instruments in vector $\boldsymbol{Z}_{it}$ as the regressors in the first stages.

$$P_{it} = \pi_0 + \pi_1 \boldsymbol{Z}_{it} + \Pi \mathbf{X}_{\mathrm{ict}} + \theta_t + \delta_{ic} + \epsilon_{ict} \quad (9)$$

$$\ln(\mathrm{NC}_{\mathrm{ict}}) = \pi_0 + \pi_1 \boldsymbol{Z}_{it} + \Pi \mathbf{X}_{\mathrm{ict}} + \theta_t + \delta_{ic} + \epsilon_{ict} \quad (10)$$

$$\ln(\mathrm{NC}_{\mathrm{ict}}) = \pi_0 + \pi_1 \sum_{t=z-2}^{z} \boldsymbol{Z}_{it} + \Pi \mathbf{X}_{\mathrm{ict}} + \theta_t + \delta_{ic} + \epsilon_{ict} \qquad (11)$$

As a robustness check, we use a control function approach, running separate first stage regressions for each of the three key endogenous variables (P_{ict}, $ln(NC)_{ict}$ and $[ln(NC)_{ict}]^2$) as well as the four endogenous covariates (S_{ict}, W_{ict}, A_{ict} and R_{ict}) on all exogenous variables then

[36] Mean count of counties that a given county *c* shares public services with, in year *t*

[37] We first run the first stage regression with $ln(NC)_{ict}$ as the dependent variable and after obtaining the predicted outcome, use the squared term of the predicted value as an instrument for $[ln(NC)_{ict}]^2$ in the second stage regression.

retrieve residuals from each regression. The second stage regressions include the residuals as additional regressors in equation (8).

5. Data and Summary Statistics

Our sample is comprised of 760 tax assessing towns and cities in New York State, 78 of which participated in 38 CAPs between 2003 and 2014. We exclude Tompkins and Nassau county that conduct property assessment at the county level. A third county, Montgomery centralized assessment to the county level in 2018, which is outside the sample period. All expenditure data are inflation adjusted to 2003 values. Table 4 lists the year each CAP was formed and dissolved during the sample period.

We use annual expenditure and tax revenue data from the New York Local Financial Data published by the New York State Comptroller's Office. We match these data to a rich set of jurisdiction-level administrative information related to assessment behaviors and local environment from the New York Office of Real Property Tax Services (ORPTS). Data provided by the ORPTS include logged total assessed value of exempt parcels, parcel count by property class, number of exempt parcels per property class, reassessment ratio, year of reassessment and annual records of state aid receipt for property assessment by program.

The New York state Office of Real Property Tax Services (ORPTS) only reports COD for a sample of assessing units that have not conduced revaluation over the past three years prior to the market value survey year. We also construct performance variables using parcel level sales data from annual *New York Market Value Survey*. For this study, we use the annual market value survey data to calculate an annual COD for each assessing unit. We restrict our parcel level data to arms-length sales so as to exclude outliers from anomaly. Then we focus on three major sub-classes of residential property, which are one-family year-round residence, rural residence with acreage, and two-family year-round residence. Together, these three classes account for a very high (to add %) percentage of the data, that is, they are the most representative. From these three classes, we develop two measures for use in the tests, one is of all three classes and the other is only of single-family year-round residences. We normalize COD in the(?) negative natural logarithm form, due to the skewed distribution of COD and for the ease of interpretation, so that a positive coefficient on $\boldsymbol{S}_{it}$ would suggest improvement in assessment uniformity.

We retrieve records of inter-municipal cooperation from the Division of Local Government and School Accountability within the Office of the NY State Comptroller. The state aid program for inter-municipal cooperation was initiated in 2005, therefore we are able to observe cases of collaboration in public service provision only from 2005 to 2014. We also calculate border contingency among jurisdictions using the ArcMap10 software and the civil boundaries shape file provided by the NYS GIS Clearinghouse.

We collect median value housing price from the Market Value Survey data for constructing median tax share variable. For demographic variables, we use population density data from American Housing Survey and population growth from the New York State Comptroller's office. County level unemployment rate is provided by the Bureau of Labor Statistics and county level private sector wage is from the New York Department of Labor. Tables 1 and 2 each provide a list of data sources and summary statistics of variables used in this study.

Table 3 provides the descriptive statistics of change in assessment costs among 78 jurisdictions that participated in CAP units in our sample period. The total assessment costs (aggregate, not per parcel) seem to be low, on average, after combining their assessing functions with their neighbors by participating in a CAP. Most of this reduction in total costs seems to be driven by cost savings in personnel cost, while contractual expenses increase. There also seems to be lagged effects, where initially the total costs increase in the first year of participating in a CAP, possibly due to various adjustment costs. The CAP participants seem to experience cost saving between the second and the third year. Appendix Table 1 lists the total, personnel and contractual costs by each jurisdiction that participated in a CAP.

6. Results

6.1. Instrument Validity

In order to get unbiased estimate of returns to scale, we first examine whether the instruments employed in our analysis are valid. Valid instruments for the CAP decision and newly constructed size variables should be good predictors of the endogenous variables, but not directly determine the total expenditure on assessment. The same logic applies to the four endogenous covariates.

Figure 1 depicts the first stage relationship between each instrument and the potentially endogenous decision to join a CAP. The vertical axis on the left-hand side of each graph indicates jurisdictions' probability of joining a CAP and the second vertical axis shows the density of each instrumental variable's histogram. Panel A shows a strong positive relationship between the count of other CAPS within the same county and individual jurisdiction's decision to enter a CAP in a given year. Panel B suggests that there might be a quadratic relation between a given jurisdiction's exposure to inter-municipal cooperation for providing public service among neighboring jurisdictions and the likelihood of deciding to combine the assessing function with its neighbors. Panels C and D suggests that county-level exposure for sharing the role of public service provision as well as the third instrument, $z_{3,ict}$ are both positively correlated with a jurisdiction's probability of joining a CAP. In the main analysis, we assess whether our instruments are weak by checking conditional first-stage f-test statistics. Since we have multiple endogenous variables, we refer to the Cragg–Donald statistic and compare the f statistic with simulated critical values from Stock & Yogo (2005). The Cragg-Donald F-statistics for all 2SLS models using these multiple instruments are above the critical value and thus passed the weak instrument test.

Second, we check the exogeneity assumption. We assess whether the instruments and errors are uncorrelated in all periods and we conduct a balancing test to see whether the exclusion restriction condition holds. Table 5 shows that there is no individual or joint statistical significance between covariates and the three instruments. The same holds for other four endogenous covariates, which is not reported in this table, but available upon request. In Tables 6 and 7, we report the p values of the Hansen J test, the null hypothesis of which is that the excluded instruments are uncorrelated with the error term: We fail to reject this null hypothesis under all models. We also show whether the instruments are orthogonal to jurisdiction level characteristic, such as share of exempt, industrial and commercial parcels, population density and average tax share.

6.2. Empirical Results

Tables 6 and 7 report the OLS and 2SLS estimates of the relation between size of assessing unit and total assessment costs. In all 2SLS models, the size of assessing unit, a quadratic term of the size variable, a dummy indicator of decision to join a CAP are all instrumented. Log of assessor wage per parcel, log of state aid and reassessment frequency as well as negative log of COD are also treated as endogenous variables in the 2SLS models. The first three columns show the effect estimates in the year a given jurisdiction joined a CAP, while the next three columns show the cost effect a year after participating in a CAP. Similarly, the last three columns show the lagged effects after two years.

The results in Table 6 shows that that assessment costs increase with assessing unit size at a decreasing rate, which is significant at 95% confidence level. The main difference across different specification is the magnitude of coefficients on size variables that leads to variation in the marginal effect of size and the turning point. In the baseline 2SLS model in column (2) for instance, an additional percent increase in size is positively associated with total cost for assessing units that have fewer than 3,385 parcels, while the marginal effect of size becomes negative for assessing units that have more than 3,385 parcels. In other words, assessing units whose sizes are smaller than the turning point experience diseconomies of scale, while the larger units are the ones that benefit from economies of scale. 3,385 parcels is less than the mean, which is 3,936 and lies between the median and top quartile in the distribution of total number of parcels within each assessing unit. This figure is also relatively larger than the average number of parcels among jurisdictions that ever participated in CAPs (which is 2,105) and smaller than the average count among jurisdictions that never participate in CAPs during our sample period (which is 3,597).

Figure 2 illustrates the marginal effect of assessing unit's size on assessment cost, graphically. These graphs help us to see the time trend or potential lagged effects of CAP participation and compare effect estimates across different specifications. Panel A summarizes the effect estimates from the first three columns in Table 6 and 7. The negative relationship between assessment cost and size of assessing unit is more pronounced among 2SLS estimates. The bias from OLS estimates appears to attenuate the estimates of diseconomies of size. The findings also hold across three years since joining a CAP.

Panels B to D each show the 2SLS estimates across three post years for different specifications: Each panel show the estimates from the baseline 2SLS models (columns 2, 5, 8 in Tables 6 and 7), models including linear trend (columns 3, 6, 9 in Tables 6 and 7) and from models using three year average instead of yearly values of instrumental variables (from Table 8). The baseline estimates suggest larger lagged effects with steeper negative slope a year after than the year a given jurisdiction joined a CAP. The turning point is also the smallest in the first year: This indirectly implies that on average, more assessing units experience economies of scale in the first year than the following year. The effect estimates also remain relatively stable when using three year average values of the three instrumental variables for CAP ($Z_{1,ict}, Z_{2,ict}, Z_{3,ict}$) instead of yearly values. The estimates using three year average values of instruments as illustrated in Panel C, Figure 2 suggest that the marginal effect is larger in the year after a given jurisdiction enters a CAP, relative to the first year.

On the other hand, when controlling for jurisdiction-specific linear trend, the estimate of the marginal effect of size on assessment cost is relatively smaller than that from other specifications, in the year a jurisdiction joined a CAP, as shown in Panel A in Figure 2[38]. Estimates in columns (3), (6) and (9) in Table 6 are from models that include jurisdiction-specific linear time trend, to account for local level characteristics in each jurisdiction that change in a linear way and correlated with both assessment costs and the decision to enter a CAP. Inclusion of such linear time trend do not affect the statistical significance of coefficients on the

[38] Also, the marginal effects show to be largest in the year a jurisdiction joined a CAP rather than later years.

size variables across most specifications, although the turning point is relatively smaller than other 2SLS estimates in Table 6. However, in Table 7, β_1 and β_2 estimates are no longer statistically significant when we include jurisdiction-specific linear time trend, in the year of joining a CAP and two years after. Not including the linear time trend would only bias the coefficients of size variables and P_{ict}, should there be a systematic relationship between the trend in assessment costs and participation in a CAP. For instance, we should be concerned of omitted variable bias if jurisdictions enter a CAP, largely due to rising trend in assessment budget, which would lead to a positive bias of our estimates. However, the coefficients of P_{ict} (dummy indicator of entering a CAP) show to be larger when controlling for jurisdiction time trend as shown in column (3) in Table 7, which is at odds with the aforementioned concern.

7. Conclusion

In this essay, we test whether larger tax assessing units may be able to save costs for property assessment. We utilize the expansion in size of assessing unit (or total count of parcels) among assessing jurisdictions that combine their assessment function with their neighbors as main source of variation to estimate the returns to scale in property assessment. Based on the economic theory of cost minimization we adapt the cost function framework widely used in the school and fire district consolidation literature. We address the potential selection bias of each jurisdiction's decision to form a coordinated unit with multiple instruments, which are constructed by using spatial intersection of jurisdiction boundaries and inter-municipal cooperation among neighboring jurisdictions for providing other types of public services.

We find significant and unbiased evidence of diseconomies of scale among small units and economies of scale among larger units. The positive association between total assessment cost and size among smaller units suggest that the increases in adjustment costs. The potential mechanism for diseconomies of scale for smaller units may be either through higher transportation costs or changes in other assessment behaviors such as more frequent reassessment or shifting from contracting out to conducting in-house assessment. Although we do not fully unpack potential mechanisms in this paper, the descriptive statistics suggest that the initial adjustment costs during the first three years are higher for smaller assessing units. On the other hand, assessing units that become sufficiently large (ranging from 3,385 to 4,355) post-CAP, benefit from positive economies of scale.

In this essay we have explored an alternative policy for small tax assessing jurisdictions aside from consolidation which is politically costly. The presence of economies or diseconomies of size has important policy implications on the design of local property tax system, but also for collaborative governance among local jurisdictions.

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Table 1. Variables and data source

Variables	Source
Performance measure(S): Assessment quality	
Calculated COD	NY Market Value Survey
Resources (Expenditure and Revenue)	
Assessment Budget	
- Assessment operation (C) (Personal services, assessors fees, equipment & capital outlay, contractual expenses, employment benefits)	
- Real property tax levy	State Comptroller's Office
- State aid for real property tax	
- State aid for coordination/consolidation	
Institutional variables	
Level of assessment: locally reported AR	
State Equalization rate	
Log assessed value of exempt properties	ORPTS
Reassessment activity: frequency, dummy, first year	
Method of revaluation: CAMA, Appraise	
Property tax levy, nominal rate	
Parcel counts per class: Residential, commercial, industry, agricultural	ORPTS
Environment variables (E)	
Median house values as share of median income	Census, NY Market Value Survey
Population growth rate (annual)	Census Intercensal dataset
Population density	American Housing Survey
Share of each property classes (annual)	ORPTS
Number of sales of single family houses	NY Market Value Survey
Log of county average wage per industry	NY Department of Labor
County unemployment rate	Bureau of Labor Statistics

Table 2. Summary Statistics

(Unit: Inflation adjusted $)	2005		2011	
	CAP	Non-CAP	CAP	Non-CAP
Assessment cost (per capita)				
Total assessment	17.80	24.09	16.88	21.51
Operating	12.19	18.66	11.63	17.64
Personnel	11.49	18.04	11.31	17.30
Contractual expense	5.61	5.42	5.24	3.86
State aid (per capita)				
Any assessment state aid	2.75	1.75	1.37	0.66
Frequent reassessment aid	0.44	0.46	0.10	0.24
County aid	0.007	0.004	0.005	0.001
Revenue and other spending				
Property tax levy	499	674	392	555
Non-assessment expenditure	1,132	1,616	1,048	1,547
	CAP	Non-CAP	CAP	Non-CAP
Assessment outcome				
COD	29.77	36.02	24.75	28.93
State EQR	89.19	72.24	92.56	73.71
Residential AR	82.23	64.99	91.26	72.39
Environment				
Agricultural/Total (%)	7.25	4.41	6.70	3.90
Residential/Total (%)	58.80	62.87	59.12	63.27
Commercial/Total (%)	3.03	3.79	2.93	3.76
Industrial/Total (%)	0.84	0.67	0.95	0.67
Exempt parcels/Total (%)	12.14	15.95	12.20	15.59
Population density	173.91	508.02	155.29	365.41
Total parcel	2,298	4,460	2,307	3,667
Median household income ($)	53,046	55,697	48,482	50,487
Median house value ($)	112,208	102,521	99,162	96,061

Table 3. Change in assessment costs among CAP participants

(Unit: Inflation adjusted $)	Assessment costs			
	Total assessment	Personnel	Contractual expense	# of parcel
Before	43,087	31,622	8,634	2,915
After (all post year average)	35,723	24,462	10,476	5,503
One year after	51,072	31,235	13,597	
Two years after	41,476	29,832	11,163	
Three years after	41,912	29,838	11,848	

Table 4. Creation of coordinated units over time

Name of Coordinated unit(CAP)	Start year	End year	# of muni
Allegany County C.A.P. #2	2002 (7)	2018	2
Cayuga County C.A.P. #2		2016	2
Herkimer County C.A.P. #2			3
Madison County C.A.P. #2			3
Ontario County C.A.P. #1		2008	2
Warren County C.A.P. #3		2014	2
Washington County C.A.P. #1		2013	2
Nassau County Assessing Unit	2003 (3)		5
Schoharie County C.A.P. #2			3
Schuyler County C.A.P. #2			5
Hamilton County C.A.P. #1	2004		3
Genesee County C.A.P. #1	2005		3
Lewis County C.A.P. #1	2006 (3)	2019	2
Livingston County C.A.P. #3		2018	2
Washington County C.A.P. #2		2016	2
Allegany County C.A.P. #3	2007 (15)	2008	2
Cattaraugus County C.A.P. #2		2014	2
Delaware County C.A.P. #2			2
Dutchess County C.A.P. #1			2
Dutchess County C.A.P. #2			2
Dutchess County C.A.P. #3			2
Genesee County C.A.P. #2			2
Genesee County C.A.P. #3			2
Jefferson County C.A.P. #1			2
Jefferson-Lewis County CAP #2		2019	2
Lewis County C.A.P. #2		2014	2
Livingston County C.A.P. #4			2
Madison County C.A.P. #3			4
Niagara County C.A.P. #1			2
Orleans County C.A.P. #2			2
Chautauqua County C.A.P. #2	2008 (10)		3
Chemung-Tioga County C.A.P. #1		2015	3
Columbia County C.A.P. #1			2
Erie County C.A.P. #1		2014	2
Essex County C.A.P. #1		2014	3
Jefferson County C.A.P. #3		2014	2

Lewis County C.A.P. #3			2
Montgomery County C.A.P. #1			2
Schuyler County C.A.P. #3		2010	2
Schuyler County C.A.P. #4		2014	2
Genesee County C.A.P. #4	2009		2
Onondaga County C.A.P. #3	(2)		2
Onondaga County C.A.P. #4	2010		2
Schuyler County C.A.P. #1	(2)		5
Cattaraugus County C.A.P. #3	2011		2

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Table 5. Validity of instruments for the decision to join a CAP**:** Balance test

	(1)	(2)	(3)
DV:	$Z_{1,ict}$	$Z_{2,ict}$	$Z_{3,ict}$
Share of exempt parcels	-10.4909	69.724	2.1901
	(6.9835)	(19.646)	(1.4473)
Share of commercial	-1.0356	-9.292	-0.6479
	(2.7864)	(13.701)	(0.4975)
Share of industrial	0.7209	-41.359	3.4162
	(12.5966)	(54.205)	(2.9287)
Population density	-0.0002	-0.00106	-0.0001
	(0.0005)	(0.00078)	(0.0001)
Mean tax share	-0.8383	6.597	-0.3332
	(1.9426)	(5.184)	(0.2185)
Year f.e.	Y	Y	Y
Jurisdiction f.e.	Y	Y	Y
F test	0.48	8.03	3.60

Note: Total number of observations is 8,464 (851 municipalities).

Table 6. Estimates from OLS and 2SLS models I

DV:	Same year as CAP $\ln(\text{Cost/NCAP}_{ict})$			One year after CAP $\ln(\text{Cost/NCAP}_{ict+1})$			Two years after CAP $\ln(\text{Cost/NCAP}_{ict+2})$		
	OLS	**2SLS**		**OLS**	**2SLS**		**OLS**	**2SLS**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\boldsymbol{Ln(NCAP)}$	1.177**	4.405**	5.217**	1.162**	6.100***	5.460**	1.286***	4.671***	5.6480**
	(0.4788)	(2.139)	(2.292)	(0.474)	(2.252)	(2.319)	(0.384)	(1.755)	(2.6302)
$\boldsymbol{Ln(NCAP)^2}$	-0.069**	-0.271**	-0.329**	-0.072**	-0.364***	-0.354**	-0.078***	-0.281***	-0.3825**
	(0.0305)	(0.1304)	(0.142)	(0.030)	(0.135)	(0.155)	(0.024)	(0.106)	(0.1656)
Turning point (N)	5,059	3,385	2,775	3,195	4,355	2,235	3,803	4,069	1,608
Year f.e.	Y	Y	Y	Y	Y	Y	Y	Y	Y
Jurisdiction f.e.	Y	Y	Y	Y	Y	Y	Y	Y	Y
Linear trend			Y			Y			Y
Cragg-Donald Fstat		10.938	12.508		8.669	7.938		7.478	13.244
Hansen J pvalue		0.074			0.1210			0.136	
Endogenous pvalue		0.153			0.355			0.512	
Observations		8,466			8,449			7,737	

Note: Other endogenous variables included in the 2SLS models are assessor wage per parcel, state aid, reassessment frequency and COD. Total number of observation are 8,466 (851 unique municipalities). Standard errors clustered at the jurisdiction level reported in parentheses. Exogenous covariates from the models include Share of exempt parcels, Share of commercial parcels, Share of industrial parcels, population density, population growth and Median tax share.
*** p<0.01, ** p<0.05, * p<0.1

Table 7. Estimates from OLS and 2SLS models II

DV:	Same year as CAP $\mathrm{Ln(Cost/NCAP_{ict})}$			One year after CAP $\mathrm{Ln(Cost/NCAP_{ict+1})}$			Two years after CAP $\mathrm{Ln(Cost/NCAP_{ict+2})}$		
	OLS	**2SLS**		**OLS**	**2SLS**		**OLS**	**2SLS**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\boldsymbol{Ln(NCAP)}$	1.177**	5.829**	1.963	0.7070	5.829**	3.926*	1.396***	4.958**	3.016
	(0.478)	(2.932)	(3.384)	(0.4669)	(2.932)	(2.178)	(0.392)	(2.456)	(2.922)
$\boldsymbol{Ln(NCAP)^2}$	-0.069**	-0.400**	-0.151	-0.0464	-0.400**	-0.260**	-0.078***	-0.339**	-0.224
	(0.030)	(0.181)	(0.203)	(0.0299)	(0.181)	(0.130)	(0.024)	(0.153)	(0.174)
$\boldsymbol{P_{ict}}$	-0.070	0.944**	3.508*	0.0409	0.944**	2.586	-0.108	0.807**	21.202
	(0.062)	(0.432)	(2.103)	(0.0817)	(0.432)	(7.241)	(0.067)	(0.395)	(34.112)
Turning point (N)	5,059	1,455	2,713	3,195	1,455	1,900	3,803	1,501	
Year f.e.	Y	Y	Y	Y	Y	Y	Y	Y	Y
Jurisdiction f.e.	Y	Y	Y	Y	Y	Y	Y	Y	Y
Linear trend			Y			Y			Y
Cragg-Donald Fstat		7.243	6.087		7.068	7.987		7.995	8.617
Hansen J pvalue		0.942			0.127			0.837	
Endogenous pvalue		0.049			0.093			0.095	
Observations		8,466			8,449			7,737	

Note: Other endogenous variables included in the 2SLS models are assessor wage per parcel, state aid, reassessment frequency and COD. Total number of observation are 8,466 (851 unique municipalities). Standard errors clustered at the jurisdiction level reported in parentheses. Exogenous covariates from the models include Share of exempt parcels, Share of commercial parcels, Share of industrial parcels, population density, population growth and Median tax share. *** p<0.01, ** p<0.05, * p<0.1

Table 8. Robustness checks

Panel A. Estimates from 2SLS models with IVs as 3Y average values

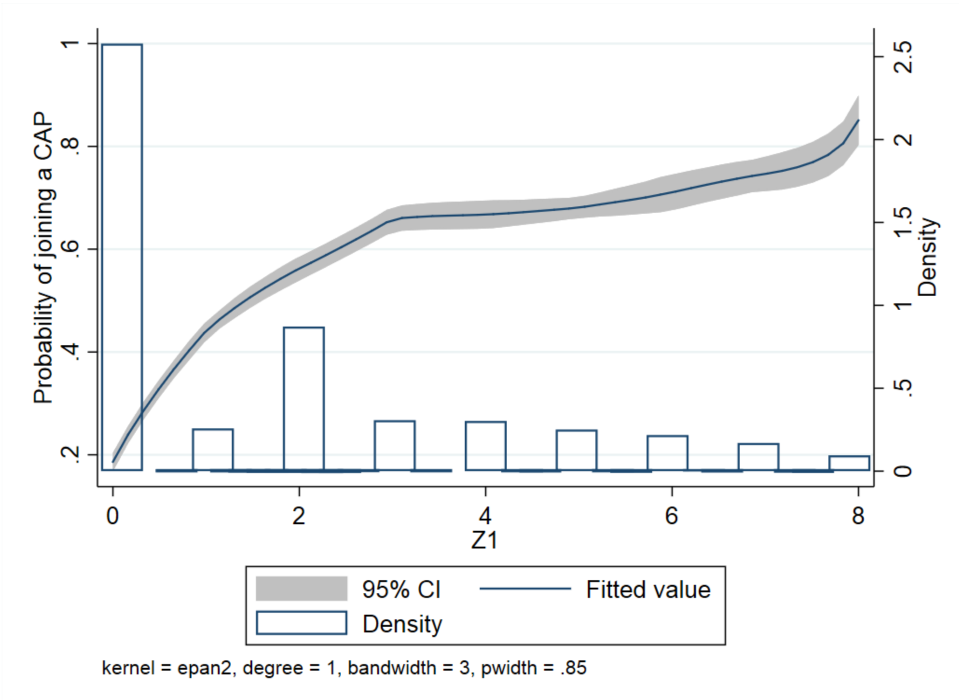
DV:	$\text{Ln}(\text{Cost}/\text{NCAP}_{ict})$		$\text{Ln}(\text{Cost}/\text{NCAP}_{ict+1})$		$\text{Ln}(\text{Cost}/\text{NCAP}_{ict+2})$	
	(1)	(2)	(3)	(4)	(5)	(6)
$\boldsymbol{Ln(NCAP)}$	5.387**	3.492	6.538***	7.117**	4.120**	5.765**
	(2.324)	(2.458)	(2.530)	(3.156)	(1.712)	(2.760)
$\boldsymbol{Ln(NCAP)^2}$	-0.325**	-0.252*	-0.390***	-0.467**	-0.249**	-0.384**
	(0.138)	(0.147)	(0.150)	(0.196)	(0.107)	(0.173)
$\boldsymbol{P_{ict}}$		0.972*		0.898		0.858*
		(0.547)		(0.601)		(0.494)
Turning point (N)	3,975	1,526	4,368	2,019	3,917	1,791
Cragg-Donald F-stat	11.217	7.995	8.284	6.303	13.366	5.798
Hansen J p-value	0.381	0.837	0.118	0.179	0.268	0.742
Endogenous p-value	0.306	0.095	0.306	0.098	0.524	0.117
Observations	8,466		8,449		7,737	

Panel B. Robustness check: Control function estimates

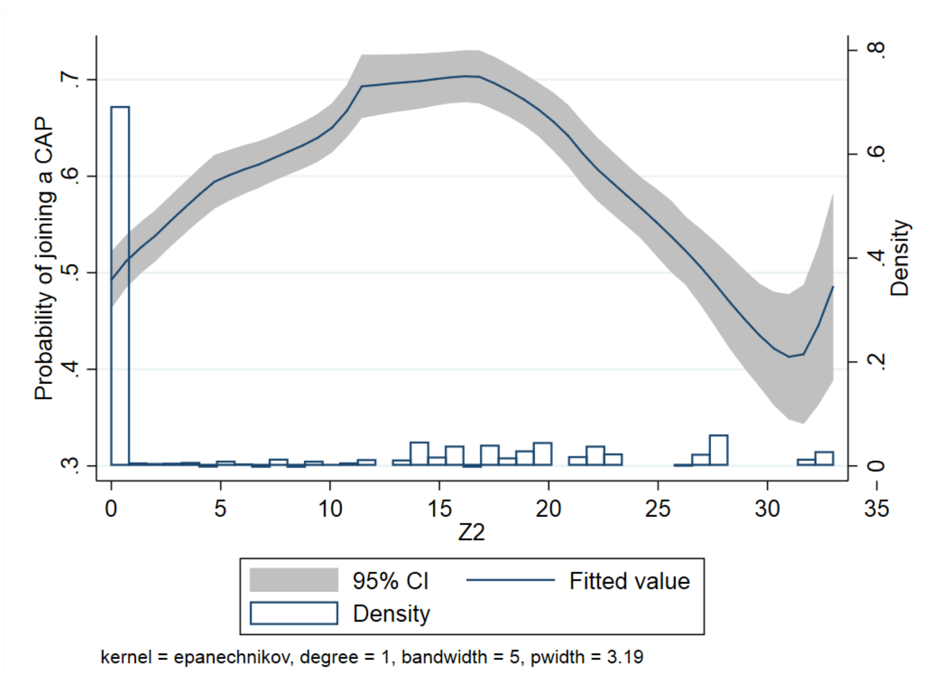
DV:	$\text{Ln}(\text{Cost}/\text{NCAP}_{ict})$	$\text{Ln}(\text{Cost}/\text{NCAP}_{ict+1})$	$\text{Ln}(\text{Cost}/\text{NCAP}_{ict+2})$
	(1)	(2)	(3)
$\boldsymbol{Ln(NCAP)}$	1.5999***	0.8497**	1.2960***
	(0.3862)	(0.3848)	(0.4713)
$\boldsymbol{Ln(NCAP)^2}$	-0.0800***	-0.0507**	-0.0830***
	(0.0224)	(0.0245)	(0.0297)
Observations	8,466	8,449	7,737

Note: All models include year and jurisdiction fixed effects/ Standard errors clustered at the jurisdiction level. *** p<0.01, ** p<0.05, * p<0.1

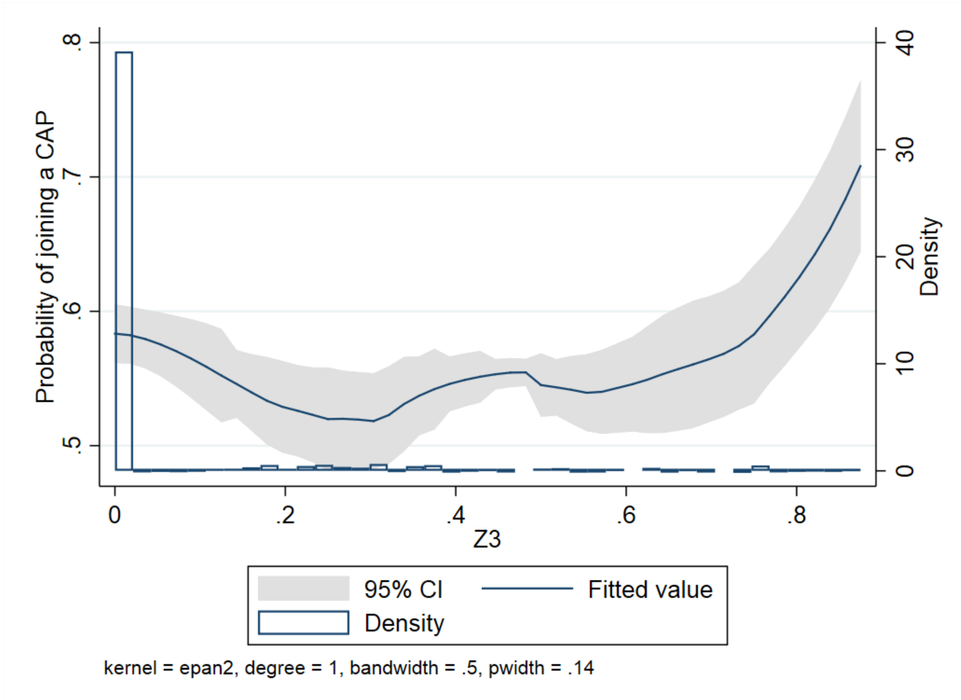
Figure 1. First stage: Relation between instruments CAP decision and the likelihood of joining a CAP



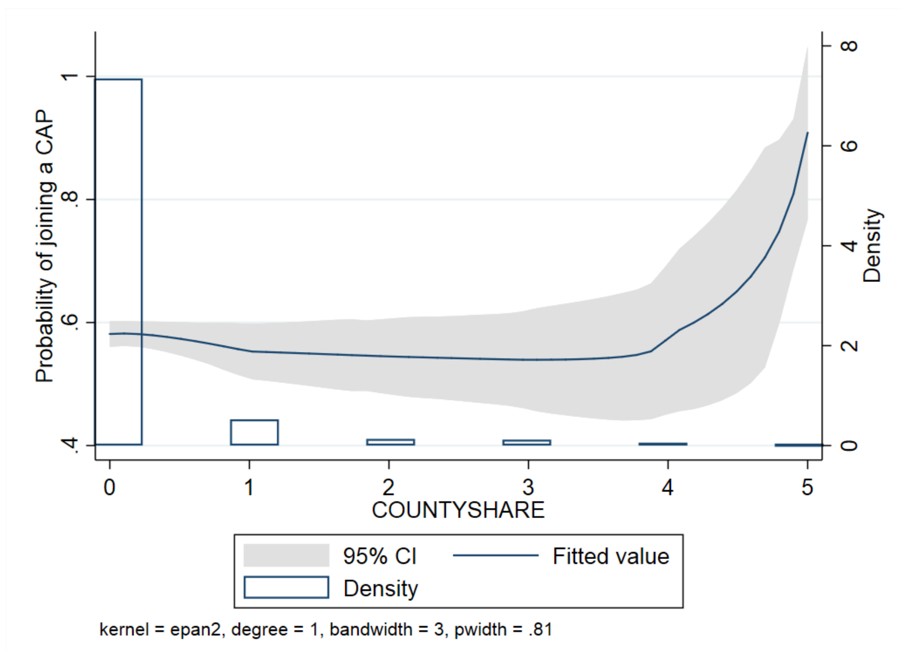
Panel A. $Z_{1,ict}$ (count of other CAP) as instrument



Panel B. $Z_{2,ict}$ as instrument

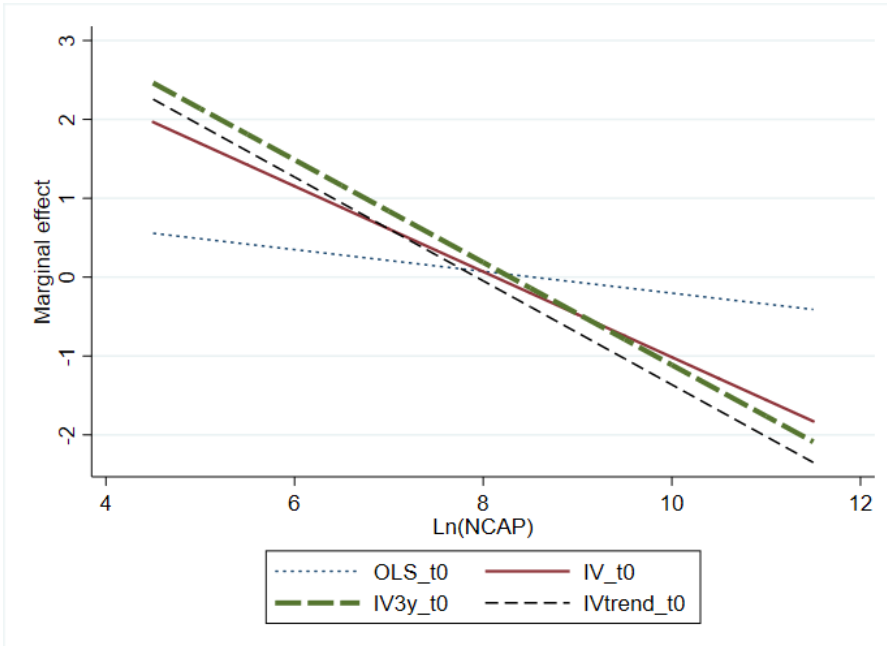


Panel C. $Z_{3,ict}$ as instrument

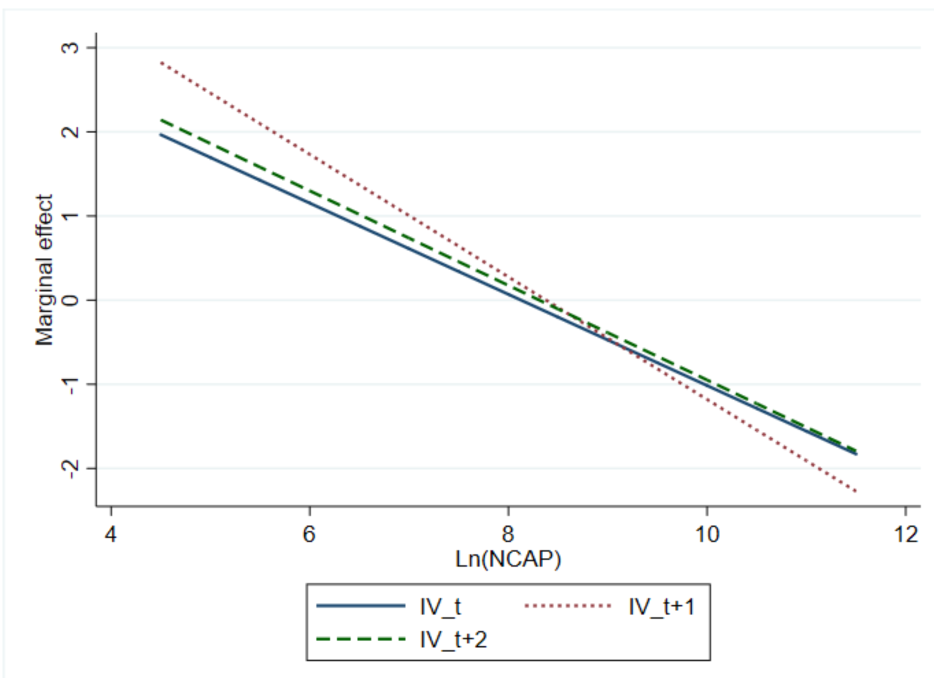


Panel D. $\mathrm{COUNTYSHARE}_{ict}$

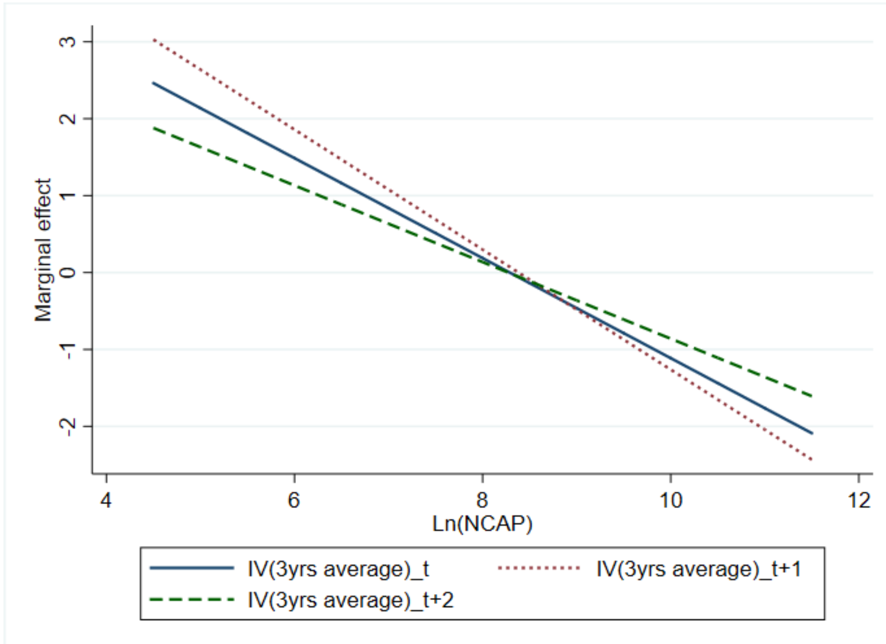
Figure 2. Marginal effects



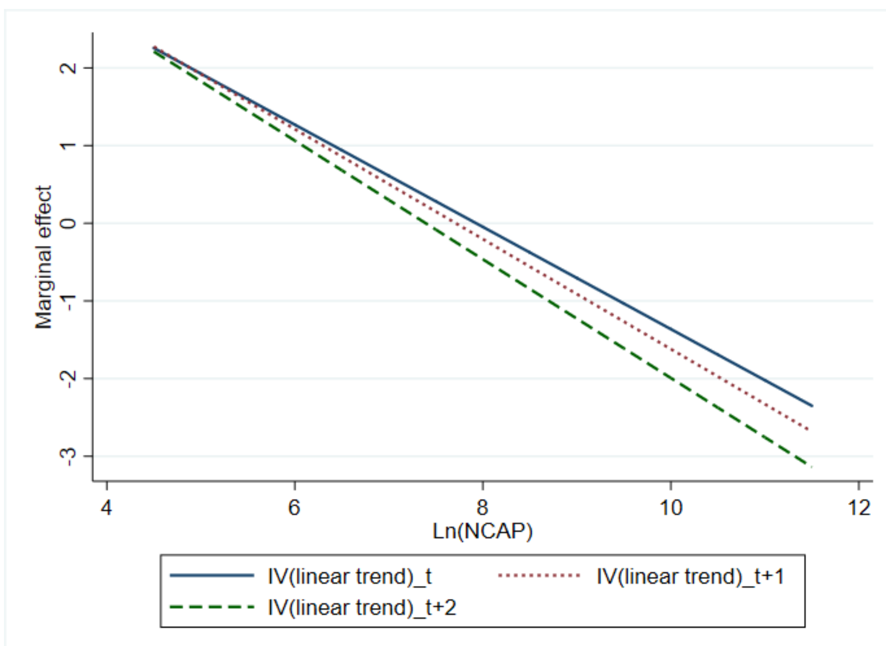
Panel A. Estimates from baseline year



Panel B. Specification 1 with lagged effects



Panel C. Specification 1I with lagged effects



Panel D. Specification III with lagged effects

Appendix

Table A1. List of municipalities participating in CAPS (78 jurisdictions in 38 CAPs)

Municipality	Start Year	End Year	Last reassess_t	Reassess years	N_{t-1}	N_t	Total spending ($ 2016) Total $_{t-1}$	Total $_t$	Total $_{t+1}$	Total $_{t+2}$
ALLEGANY ALMA Town	2002	2018	2002	2002-2007	1,047	5,184	4,523	16,454	404	9,058
ALLEGANY WELLSVILLE Town	2002	2018	2002	2000-2007	3,777	5,184	44,260	104,578	70,842	55,089
ALLEGANY ALLEN Town	2007	2008	2007	05-07,09-11	564	1,145	4,875	4,925	4,900	5,187
ALLEGANY BIRDSALL Town	2007	2008	2007	2001-2011	568	1,145	4,714	5,452	11,785	5,966
CATTARAUGUS ELLICOTTVILLE Town	2007	2014	2007	2000,05-12	2,798	5,990	55,834	42,454	65,816	73,129
CATTARAUGUS ALLEGANY Town	2007	2014	2007	2004-2012	3,167	5,990	77,768	79,186	80,782	65,664
CATTARAUGUS HINSDALE Town	2011		2010	2007-2010	1,417	2,181	15,905	15,511	14,895	14,479
CATTARAUGUS ISCHUA Town	2011		2010	2010	756	2,181	7,166	7,060	7,062	7,215
CAYUGAIRA Town	2002	2016	2000	2003-2011	1,214	2,144	10,311	10,165	10,729	13,463
CAYUGA VICTORY Town	2002	2016	1997	2003-2011	912	2,144	5,969	24,592	20,225	21,331
CHEMUNG CHEMUNG Town	2008	2017	2008	08-11,13,15	1,456	6,565	48,555	43,771	21,678	24,045
CHEMUNG VAN ETTEN Town	2008	2017	2008	08-11,13,15	11,49	6,565	27,954	14,384	9,620	9,867
TIOGA BARTON Town[39]	2008	2013	2008	08-11,13,15	3,966	6,565	58,306	52,692	36,549	44,466
COLUMBIA AUSTERLITZ Town	2008		2005	2011,13-15	1,500	3,142	20,208	23,320	17,864	19,249
COLUMBIA HISSDALE Town	2008		2005	2011,13-15	1,635	3,142	26,698	29,963	30,416	73,762
DELAWARE KORTRIGHT Town[40]	2007		2007	07-11,13-15	1,601	2,937	17,243	16,034	18,929	18,697

[39] Pre CAP period: Annual reassessment in 2002, 2003 ; initial assessment in 1985
[40] Pre CAP period: Annual reassessment in 2005-2006 ; initial assessment in 2001

DELAWARE MEREDITH Town	2007		2007	08-11,13-15	1,317	2,937	16,407	15,892	18,668	18,582
DUTCHESS FISHKILL Town	2007		2008		7,041	16,842	386,099	228,349	202,435	225,016
DUTCHESS WAPPINGER Town	2007		2007		9,381	16,842	262,605	234,002	149,415	119,736
DUTCHESS EASTFISHKILL Town	2007		2007	2007-2015	11,403	15,971	401,104	461,920	231,866	210,274
DUTCHESS BEACON City	2007		NA	NA	4,470	15,971	119,968	136,927	82,324	79,174
DUTCHESS LAGRANGE Town	2007		2007	2007-2015	6,311	8,402	230,336	200,103	128,980	124,350
DUTCHESS UNIONVALE Town	2007		2007	2007-2015	2,014	8,402	27,458	30,144	33,425	35,020
ERIE EVANS Town	2008	2014	2008	2008-2010	9,727	11,746	256,838	230,345	117,009	108,852
ERIE NORTHCOLLINS Town[41]	2008	2014	2008	2008-2010	2,017	11,746	18,851	19,916	20,991	20,233
ESSEX ELIZABETHTOWN Town	2008		2008	2008-2015	1,409	5,002	12,000	19,843	22,711	22,336
ESSEX WESTPORT Town	2008		2008	2008-2015	1,428	5,002	28,961	52,096	53,817	61,665
ESSEX WILLSBORO Town	2008	2014	2008	2008-2014	2,157	5,002	28,508	28,006	39,721	32,534
GENESEE BETHANY Town	2005		2005	05,08,11,14	978	5,030	17,981	19,162	18,733	19,292
GENESEE DARIEN Town	2005		2005	05,08,11,14	1,740	5,030	28,446	30,812	27,504	28,481
GENESEE PEMBROKE Town[42]	2005		2005	05,08,11,14	2,281	5,030	32,609	33,200	32,974	34,307
GENESEE BYRON Town	2009		2009	2009-2015	1,340	2,866	18,645	17,349	17,787	18,750
GENESEE OAKFIELD Town	2009		NA		1,510	2,866	26,119	19,920	23,213	24,011
HAMILTON BENSON Town	2004		2004	2004-2007	587	2,657	7,820	NA	5,367	5,177
HAMILTON HOPE Town	2004		2004	2004-2007	596	2,657	12,521	13,995	6,670	7,161
HAMILTON WELLS Town	2006		2006	2006-2007	1,479	2,645	20,554	17,393	13,994	14,613

[41] Pre CAP period: Annual reassessment in 2001-2007 ; initial assessment in 1986
[42] Pre CAP period: Annual reassessment in 2002 ; initial assessment in 1976

HERKIMER COLUMBIA Town	2002		1997	2003-2007	997	2,973	4,007	8,970	13,547	23,115
HERKIMER LITCHFIELD Town	2002		1991	2003-2007	903	2,973	6,879	8,696	12,466	21,396
HERKIMER WINFIELD Town	2002		1997	2003-2007	1,051	2,973	7,497	10,314	16,514	24,124
JEFFERSON LORRAINE Town	2007		2007	2007,2012	660	1,394	6,405	7,790	7,646	7,159
JEFFERSON RODMAN Town[43]	2007		2007	2007,2012	726	1,394	10,355	9,296	11,245	8,148
JEFFERSON CHAMPION Town	2007	2019	2007	2007-2014	1,979	3,529	27,732	37,438	37,335	39,154
LEWIS DENMARK Town[44]	2007	2019	2007	2007-2014	1,504	3,529	15,552	19,738	16,362	16,569
JEFFERSON CLAYTON Town	2008	2014	2008	2008-2014	3,932	6,284	67,536	92,219	96,558	92,298
JEFFERSON ORLEANS Town[45]	2008	2014	2008	2008-2013	2,730	6,284	97,691	156,226	86,404	88,847
LEWIS NEWBREMEN Town	2007	2014	2007	2007-2013	1,597	3,935	23,008	31,313	29,761	30,564
LEWIS WATSON Town	2007	2014	2007	2007-2015	2,308	3,935	28,968	38,409	37,268	38,943
LEWIS OSCEOLA Town	2008		2008	2008-2014	866	1,402	7,003	7,325	7,281	7,200
LEWIS MONTAGUE Town[46]	2008		2008	2008-2014	535	1,402	4,874	5,623	5,481	5,219
LIVINGSTON WESTSPARTA Town	2006	2018	2006	06,10,14	828	2,469	13,906	12,750	12,607	13,301
LIVINGSTON SPRINGWATER Town[47]	2006	2018	2006	06,10,14	1,629	2,469	19,379	20,506	21,361	21,476
MADISON LINCOLN Town	2002		2002	02-05,08,12	1,031	6,864	8,253	8,313	1,189	15,493
MADISON LENOX Town	1997		2002	02-05,08,12	5,806	6,864	51,100	43,768	50,163	62,442
MADISON STOCKBRIDGE Town[48]	1997		2002	02-05,08,12	5,806	6,864	9,034	8,867	9,361	8,761

[43] Pre CAP period: Annual reassessment in 2002 ; initial assessment in 1982
[44] Pre CAP period: Annual and initial reassessment from 2003 until 2006
[45] Pre CAP period: Annual reassessment in 2002(CLAYTON) and 2006 (ORLEANS) ; initial assessment in 1982 for both
[46] Pre CAP period: Annual and initial reassessment in 2002(OSCEOLA) and 2000-2001(MONTAGUE; initial assessment in 1998)
[47] Pre CAP period: Annual reassessment in 2002; initial assessment in 1980
[48] Pre CAP period: Annual reassessment in 2000-2001 ; initial assessment in 1981

MADISON SMITHFIELD Town	2007		2006	2006,2010	775	5,339	8,872	6,838	6,948	7,222
MADISON NELSON Town[49]	2007		2006	2006,2010	1,776	5,390	15,384	15,208	15,396	26,634
MONTGOMERY CHARLESTON Town	2008		2008	08-10,15	1,143	4,268	15,794	17,362	19,506	14,991
MONTGOMERY ROOT Town[50]	2008		2008	08-10,15	1,304	4,268	17,619	15,078	20,063	21,408
NIAGARA WILSON Town[51]	2007		2006	NA	3,106	26,246	35,838	52,212	57,843	37,479
NIAGARA NIAGARAFALLS City	2007		2003	NA	23,135	26,246	474,682	557,465	427,235	433,104
ONONDAGA LYSANDER Town[52]	2009		2009	2009-2015	9,184	15,003	156,144	149,245	153,831	159,874
ONONDAGA VANBUREN Town	2009		2009	2009-2015	5,634	15,003	119,368	100,029	106,070	102,436
ONONDAGA CAMILLUS Town	2010		2010	2010-2012	10,391	10,402	194,493	167,905	182,689	171,345
ONTARIO GORHAM Town[53]	2002	2008	2002	02-11,14	2,606	4,089	55,644	51,326	57,375	55,939
ONTARIO SENECA Town	2002	2008	2002	02-13,15	1,461	4,089	37,588	31,911	39,670	43,102
ORLEANS GAINES Town[54]	2007		2007	07,10,13	1,258	5,237	17,820	577	0	0
OSWEGO ALBION Town	2007		2007	2007-2012	1,426	5,237	13,587	14,748	14,868	25,215
SCHOHARIE CARLISLE Town	2003		NA	NA	1,023	3,411	8,780	9,330	10,116	9,335
SCHOHARIE SEWARD Town	1996		NA	NA	2,372	3,411	9,733	9,769	9,941	9,635
SCHOHARIE SHARON Town	1996		NA	NA	2,372	3,411	11,006	11,586	11,794	11,208
SCHUYLER CAYUTA Town[55]	2008	2010	2008	2008-2015	413	8,016	3,911	7,125	7,896	6,373

[49] Initial assessment in 1981
[50] Pre CAP period: Annual reassessment in 2006-2007 (CHARLESTON ; initial assessment in 2006) and 2002-2007 (ROOT ; initial assessment in 1982)
[51] Pre CAP period: Annual reassessment in 2000-2006 (WILSON ; initial assessment in 1982) and 2003 (NIAGARAFALLS ; initial assessment in 1983)
[52] Pre CAP period: Annual reassessment in 2000-2008 ; initial assessment in 1988 and 1989
[53] Pre CAP period: Annual reassessment in 2000-2001 ; initial assessment in 1997 and 1999
[54] Pre CAP period: Annual reassessment in 2001 and 2004(GAINES; initial in 1980) and 2000-2006 (ALBION; initial assessment in 1998)
[55] Pre CAP period: Annual reassessment in 2002, 2007 (HECTOR; initial in 1990); in 2002(CAYUTA and TYRONE; initial in 1996) and 2000-2002 (MONTOUR; initial assessment in 1999)

SCHUYLER HECTOR Town	2008	2010	2008	2008-2015	3,498	8,016	74,605	40,987	51,436	54,417
SCHUYLER TYRONE Town	2008	2014	2008	2008-2015	1,698	8,016	24,336	19,953	2,451	28,112
SCHUYLER CATHARINE Town	1999	NA	2010	2010-2015	2,364	8,016	16,233	12,016	11,801	12,868
SCHUYLER MONTOUR Town	1999	2018	2008	2008-2015	2,364	8,016	17,466	13,223	13,105	14,127
SCHUYLER DIX Town[56]	2003		2002	2009-2015	2,174	4,594	26,717	28,373	29,570	31,326
SCHUYLER READING Town	2003		2002	2009-2015	1,234	4,594	16,197	16,213	17,412	17,943
SCHUYLER ORANGE	2008	2014	2002	2008-2015	1,206	4,752	18,967	NA	NA	19,092
ORANGE CHESTER Town[57]	2002	2014	NA	NA	4,766	11,105	102,992	106,757	112,392	127,412
WARREN CHESTER Town	2002	2014	2002	02,04,08	3,769	11,105	95,992	99,202	71,191	85,017
WARREN HORICON Town	2002	2014	2002	02,04,08	2,481	11,105	71,4680	53,749	37,665	54,436
WASHINGTON DRESDEN Town[58]	2002	2013	2001	NA	856	1,859	17,640	15,491	15,667	16,696
WASHINGTON PUTNAM Town	2002	2013	2001	2013	1,000	1,859	17,596	15,888	13,592	14,544
WASHINGTON GREENWICH Town	2006	2016	NA	NA	2,500	2,506	35,071	36,716	39,232	39,845
WASHINGTON KINGSBURY Town[59]	2006	2016	2006	2005-2015	4,723	4,735	70,709	73,048	76,971	82,927

[56] Pre CAP period: Annual reassessment in 2002 (DIX; initial in 1992); Initial assessment in 2001 (ORANGE) and 2002(READING)
[57] Initial assessment in 1989 (Town of CHESTER) and in 1993 (Town of HORICON)
[58] Initial assessment in 1997 in both towns
[59] Pre CAP period: Annual reassessment in 2000-2001,2003-2004 and initial assessment in 1998

Table A2. Assessment cost by category among CAP participants

Municipality	Start Year	*Personnel costs* Pre (t-1)	Post (t)	Post(t+1)	*Contractual expenses* Pre (t-1)	Post (t)	Post(t+1)
ALLEGANY ALMA Town	2002	4,140	0	0	383	16,454	404
ALLEGANY WELLSVILLE Town[60]	2002	39,336	45,676	47,328	4,199	9,785	4,588
ALLEGANY ALLEN Town	2007	4,750	4,750	4,750	125	175	150
ALLEGANY BIRDSALL Town[61]	2007	4,300	4,636	0	414	816	11,785
CATTARAUGUS ELLICOTTVILLE Town	2007	24,777	25,212	28,539	31,057	17,242	37,278
CATTARAUGUS ALLEGANY Town	2007	12,328	11,487	4,643	64,240	66,789	75,439
CATTARAUGUS HINSDALE Town	2011	12,300	12,878	12,878	3,605	2,633	2,017
CATTARAUGUS ISCHUA Town	2011	7,000	7,000	7,000	166	60	62
CAYUGA IRA Town	2002	9,300	750	750	1,011	9,415	9,979
CAYUGA VICTORY Town	2002	5,725	12,900	12,900	244	11,692	7,325
CHEMUNG CHEMUNG Town	2008	18,966	20,780	19,405	28,845	21,513	1,833
CHEMUNG VAN ETTEN Town	2008	10,070	9,500	9,500	17,884	4,884	120
TIOGA BARTON Town	2008	46,037	32,022	28,781	12,268	16,815	7,334
COLUMBIA AUSTERLITZ Town	2008	18,287	19,463	17,670	1,921	3,857	194
COLUMBIA HISSDALE Town	2008	24,593	25,149	27,758	2,105	4,814	1,655
DELAWARE KORTRIGHT Town	2007	14,428	14,861	17,500	2,365	1,173	1,429
DELAWARE MEREDITH Town	2007	15,240	15,690	17,500	1,167	202	1,168
DUTCHESS FISHKILL Town	2007	228,780	128,608	117,442	6,305	64,741	84,992
DUTCHESS WAPPINGER Town	2007	74,405	132,632	128,038	3,690	8,537	15,816
DUTCHESS EASTFISHKILL Town	2007	135,284	188,910	185,448	38,368	37,818	46,418
DUTCHESS BEACON City	2007	26,207	34,753	38,069	92,458	101,868	44,255

[60] 0 for personnel cost from 2002 until 2015
[61] 0 for personnel cost from 2008 until 2015

DUTCHESS LAGRANGE Town	2007	109,672	117,168	122,938	33,588	5,536	6,042
DUTCHESS UNIONVALE Town[62]	2007	27,458	29,241	31,890	0	903	1,535
ERIE EVANS Town	2008	102,420	132,997	108,819	4,224	7,946	8,190
ERIE NORTHCOLLINS Town[63]	2008	0	0	0	18,851	19,916	20,991
ESSEX ELIZABETHTOWN Town[64]	2008	10,000	0	0	2,000	19,843	22,711
ESSEX WESTPORT Town	2008	26,636	50,000	51,240	2,325	2,096	2,578
ESSEX WILLSBORO Town	2008	26,869	23,240	32,526	1,639	4,766	7,195
GENESEE BETHANY Town	2005	14,740	15,182	15,637	2,996	3,820	3,096
GENESEE DARIEN Town	2005	18,820	21,000	21,500	7,130	9,812	6,004
GENESEE PEMBROKE Town	2005	31,632	32,472	32,412	977	727	562
GENESEE BYRON Town	2009	16,500	16,800	16,800	1,479	549	987
GENESEE OAKFIELD Town	2009	16,000	17,790	17,801	9,367	1,378	4,660
HAMILTON BENSON Town	2004	5,000	NA	5,000	2,820	NA	367
HAMILTON HOPE Town	2004	7,100	5,300	6,300	5,422	8,696	370
HAMILTON WELLS Town	2006	19,875	15,390	13,390	679	2,003	604
HERKIMER COLUMBIA Town[65]	2002	3,936	8,273	11,172	71	697	2,376
HERKIMER LITCHFIELD Town	2002	0	0	0	6,879	8,696	12,466
HERKIMER WINFIELD Town	2002	6,228	0	0	1,269	10,314	16,514
JEFFERSON LORRAINE Town	2007	6,200	6,800	6,800	205	990	846
JEFFERSON RODMAN Town	2007	7,500	7,725	7,725	2,855	1,571	3,520
JEFFERSON CHAMPION Town	2007	18,715	35,530	36,264	7,614	1,213	1,071
LEWIS DENMARK Town	2007	14,000	15,000	15,180	1,552	4,738	1,182
JEFFERSON CLAYTON Town	2008	7,681	25,312	13,662	58,855	66,006	82,397

[62] 0 contractual expense in 2006
[63] 0 for personnel cost from 2006 until 2013
[64] 0 for personnel cost from 2008 until 2015
[65] 0 for personnel costs for all years in LITCHFIELD Town; from 2008 until 2015 in COLUMBIA Town; from 2002 until 2015 in WINFIELD Town

JEFFERSON ORLEANS Town	2008	59,740	62,000	63,860	37,951	94,226	22,544
LEWIS NEWBREMEN Town	2007	19,128	25,621	26,010	3,880	5,692	3,751
LEWIS WATSON Town	2007	24,651	35,050	35,450	4,317	3,359	1,818
LEWIS OSCEOLA Town	2008	6,900	6,900	6,900	103	425	381
LEWIS MONTAGUE Town	2008	4,000	5,000	5,000	874	623	481
LIVINGSTON WESTSPARTA Town	2006	11,050	11,400	11,900	790	1,350	707
LIVINGSTON SPRINGWATER Town	2006	18,000	18,550	19,300	1,379	1,956	2,061
MADISON LINCOLN Town[66]	2002	2,002	753	588	7,500	7,725	0
MADISON LENOX Town	1997	45,591	37,696	33,623	4,390	3,332	14,484
MADISON STOCKBRIDGE Town[67]	1997	7,600	7,800	0	1,383	837	9,326
MADISON SMITHFIELD Town	2007	7,500	6,600	6,800	1,372	238	148
MADISON NELSON Town	2007	14,465	14,465	14,900	919	743	496
MONTGOMERY CHARLESTON Town	2008	12,525	11,000	13,700	3,269	6,362	5,806
MONTGOMERY ROOT Town	2008	12,000	12,264	12,630	5,619	2,814	7,433
NIAGARA WILSON Town	2007	21,260	15,873	19,291	14,578	36,339	38,552
NIAGARA NIAGARAFALLS City	2007	328,130	381,582	404,279	146,319	147,279	22,439
ONONDAGA LYSANDER Town	2009	124,308	121,140	117,890	31,691	28,105	35,941
ONONDAGA VANBUREN Town	2009	100,532	94,490	96,872	16,137	5,539	9,198
ONONDAGA CAMILLUS Town	2010	129,682	130,961	140,611	64,268	36,945	41,087
ONTARIO GORHAM Town	2002	47,937	43,737	47,499	7,706	7,589	5,377
ONTARIO SENECA Town	2002	28,986	26,606	34,570	5,933	5,305	4,905
ORLEANS GAINES Town[68]	2007	10,925	495	0	6,040	82	0
OSWEGO ALBION Town	2007	11,650	12,200	12,275	1,287	1,898	2,593

[66] 0 contractual expense for a single year in 2003
[67] 0 personnel cost from 2003 until 2015
[68] 0 for all costs from 2007 until 2015

SCHOHARIE CARLISLE Town	2003	8,160	8,184	8,232	620	1,146	1,884
SCHOHARIE SEWARD Town	1996	8,488	8,608	8,656	1,246	1,162	1,285
SCHOHARIE SHARON Town	1996	10,376	10,376	10,400	1,080	630	1,187
SCHUYLER CAYUTA Town	2008	5,550	5,825	6,049	1,638	1,300	1,847
SCHUYLER HECTOR Town	2008	25,587	19,727	1,436	49,018	21,260	50,000
SCHUYLER TYRONE Town[69]	2008	297	17,401	0	24,039	2,553	2,451
SCHUYLER CATHARINE Town	1999	375	375	375	15,858	11,641	11,426
SCHUYLER MONTOUR Town[70]	1999	0	0	0	17,467	13,223	13,105
SCHUYLER DIX Town[71]	2003	0	0	425	26,717	28,373	29,145
SCHUYLER READING Town[72]	2003	0	0	0	16,197	16,213	17,412
SCHUYLER ORANGE[73]	2008	13,708	NA	NA	2,071	NA	NA
ORANGE CHESTER Town	2002	91,923	95,883	99,168	11,069	10,874	12,103
WARREN CHESTER Town	2002	56,304	59,365	62,849	39,297	38,378	8,343
WARREN HORICON Town	2002	42,074	25,640	12,435	27,353	27,885	25,230
WASHINGTON DRESDEN Town	2002	14,000	14,000	14,000	3,639	1,491	1,667
WASHINGTON PUTNAM Town	2002	14,500	14,940	12,000	3,096	948	1,592
WASHINGTON GREENWICH Town	2006	29,037	30,940	32,650	5,904	5,776	6,009
WASHINGTON KINGSBURY Town	2006	64,545	67,786	69,691	6,164	5,262	7,280

[69] 0 personnel cost from 2009 until 2015
[70] 0 personnel cost for all years
[71] 0 personnel cost for all years except 2004
[72] 0 personnel cost for all years
[73] Missing budget values for 2008 and 2009; 0 personnel cost in 2010, 2013-2015

Chapter 3.

Impact of Property Assessment Frequency on Assessment Outcomes: Evidence from Virginia and New York

Yusun Kim

Department of Public Administration

Syracuse University, Syracuse, New York, 13244-1020

ykim79@syr.edu

Yilin Hou

Department of Public Administration

Syracuse University, Syracuse, New York, 13244-1020

yihou@maxwell.syr.edu

Abstract

This essay examines the effect of regular, short assessment cycles on assessment performance, using two separate case studies of assessing jurisdictions in two representative states, Virginia and New York. Infrequent property assessment is commonly known to undermine equity and the efficiency of levying property taxes. This essay first tests the effect of reassessment lag on assessment uniformity using sample of assessing jurisdictions in Virginia. Then it provides estimates of the relationship between the length of reassessment cycle and assessment uniformity from a separate sample of New York's local assessing units, by employing multiple instrumental variables constructed from parcel-level sales data. We find an average of 2% deterioration in assessment uniformity per additional year of skipping reassessment, relative to jurisdictions that reassess annually, in Virginia. We also find that longer reassessment cycle leads to deterioration in assessment uniformity among towns and cities in New York. From an event study analysis using sub-groups of assessing jurisdictions in New York that ever committed to annual reassessments for multiple consecutive years, we find evidence on annual reassessment leading to improvement in assessment quality.

Key words: property tax, assessment cycle, equity, coefficient of deviation

JEL codes: H2, H7, R51

1. Introduction

How does the length of reassessment cycle affect assessment quality? Does annual reassessment improve equity in property assessment? This paper examines the effect of the regularity or frequency of property valuation on assessment quality. We also test the conventional wisdom in property tax administration that annual reassessments are the gold standard.

One of the key challenges in property tax administration is estimating the tax base through accurate and fair assessment. Particularly, the problem arises since the market value of a vast majority of properties are not observed in most years, unless they are sold in a competitive market. The main objective of a local government's Office of Tax Assessor is to accurately estimate the value of each property based on an estimate of its market value. Inaccurate assessment leads to inequity problems, as any discrepancy in the ratio between assessment value to the fullmarket value across two properties of the same value would lead to different effective property tax rates.

In practice, there is a large variation in assessment behaviors that may contribute to inaccurate assessment. A number of empirical studies have spelled out various determinants of variation in assessment uniformity (Geraci & Plourde, 1976; Mikesell, 1980; Giertz & Chicoine, 1990; Bowman & Mikesell, 1990; Eom, 2008). One set of key determinants include individual level characteristics of assessors (such as indicator of elected versus appointed) or methods used to estimate market value. The other group of determinants are the institutional features of the property tax system, such as the frequency of assessment; fractional assessment or average assessment ratio, the level of government that conducts assessment, which is also related to the size of local assessing unit and resources available for assessment[74]. This study intends contribute to the empirical literature by isolating the effect of reassessment frequency on assessment uniformity, conditional on other assessment behaviors. So far, the consensus among scholars and property tax practitioners is almost unanimous – regular and short cycles of reassessment is the norm of equitable administration of the property tax. The ideal is considered to be annual assessment. However, a vast majority of assessing jurisdictions nationwide conduct reassessment less frequently, either voluntarily or involuntarily. Considering the high political and pecuniary costs of annual reassessments, it is important to understand whether and how reassessing less frequently than annually leads to deterioration in assessment uniformity[75].

States fall into three groups in terms of required cycles of reassessment. The first group (ten states including Georgia, Massachusetts, and Pennsylvania) of state governments require local assessing jurisdictions to reassess annually (Higginbottom 2010). On the other hand, most states (including Virginia) belong to the second group that mandate longer cycles. 18 states mandate regular cycles ranging from every two to 4 years, and 11 states require valuation to be updated at least every five to 10 years. The last group (of seven states including New York) do not mandate regular assessments[76]. In this paper, we focus on two different cases of state setting, in the Commonwealth of Virginia (VA) and New York State (NYS). Virginia is a representative example of a strong Dillon Rule State where the maximum length of reassessment cycle is fixed for all counties and independent cities that conduct property assessment. The 1984 Virginia tax

[74] Other determinants that may not be directly related to assessment behavior but may affect assessment uniformity includes local fiscal factors such as property tax limitations; effective tax rates (Bowman & Mikesell, 1978; Borland & Lile, 1980; Bowman& Butcher, 1986) and monitoring pressure of local tax payers (Eom, 2008).

[75] The net cost of reassessment may have been higher even after receipt of aid that they decided to stop investing in annual revaluation. When the town of Cicero conducted reassessment most recently for the first time in over ten years since 2006, the town had to spend approximately $1,000,000 for full revaluation to update their inventory and record of sales. However, the additional revenue the town was able to generate was not large enough to offset the cost, even with the reimbursement of $67,500 of state aid.

[76] Two others, New Jersey and California, reassess upon sale or improvement.

code also required counties and cities to adopt a regular cycle and for most localities the cycle did not change, so the timing of reassessment was pre-determined in 1984, and plausibly exogenous to the determination of assessment outcomes in the recent years. On the other hand, New York is a typical example of a strong Home Rule State where the state lets local jurisdictions choose their own cycle. Though home rule states often provide financial assistance to assessing jurisdictions to encourage frequent reassessment.

The potential contribution of the paper is two- fold: to fill in the gap in the reassessment cycle literature and to provide the first empirical evidence on the effects of short cycles that can be null, positive or otherwise. Does the recency and length of reassessment cycle affect assessment uniformity? Does commitment to annual reassessment improve assessment quality? These are the questions we plan to empirically test with two separate case studies of assessing jurisdictions in Virginia and New York. To test the first question, we employ a production function model and multiple instrumental variables that are based on the dispersion in housing price and growth rate in median market value to address the potential endogeneity in assessing jurisdictions' decision to reassess. We also use an event study framework to test the second question about the effects of annual reassessment. This paper reports an average effect of 2% reduction in assessment uniformity for each additional year without reassessment and a 4% reduction for each additional year in a cycle.

The rest of paper is organized as follows. Section 2 reviews relevant literature. Section 3 presents the background to the research question examined in the context of Virginia and New. Section 4 illustrates the data and identification strategy used in this study and Section 5 summarizes the findings.

2. Literature Review

While scholars have long debated the incidence of the real property tax, the academic literature is relatively short on administration of the tax and specifically, the effects of assessment cycle. The earlier literature identified individual assessors' characteristics along with their staffing and tools of the assessment office as the key determinants of assessment uniformity (Geraci 1977; Bowman & Mikesell 1990).

Mikesell (1980) is one of the few studies that tests the relation between the cycle of value reassessment on assessment quality. If reassessments do not improve equity, then their value is questionable. He finds the revaluation process often merely copies prior year's value, sometimes adjusted for a flat percentage for all properties and sometimes with even no change at all (except additions or sales) among 31 states that require annual reassessment. Using state-level data of 46 states, the natural log of COD as dependent variable, Mikesell (1980) regresses ETR and prescribed assessment cycle on COD. His results show that both variables exerted statistically significant negative effects. Specifically, at the median effective property tax rate, COD would be 3.4 units lower with a prescribed cycle than without a specified cycle.

An important rationale for state governments promoting frequent and preferably annual reassessment is that unavoidable assessment errors capitalize into property values and will stay and can escalate if not corrected in a timely manner. A simple capitalization model of property values based on present value calculations suggests that shorter cycles fare better than longer ones and annual cycles are the best (Mikesell, 1980). However, there is no consistent evidence to support the idea that annual reassessment should be the norm cycle. Using data of the

Commonwealth of Virginia local assessing units in years 1973 through 1976, Mikesell (1980) shows that at least 68 percent of jurisdictions that were in a cyclical reassessment year improved their uniformity (lower COD) by 10 percent. In contrast, the year-to-year improvements in the annual reassessment group is much smaller. The reason, he argues, is that when the prior assessment is more recently updated, it is more likely that mass appraisal may not lead to major improvements in uniformity.

A key limitation of these earlier studies is that the findings are largely descriptive to conclude that annual reassessment is the optimal cycle. A more recent study accounts for the potential simultaneity and omitted variable bias in the earlier estimates. Eom (2008) uses cross-sectional data of assessing towns and cities in New York in 1992, and specifies a long list of determinants in his empirical model, of assessment uniformity, instrumenting the potentially endogenous regressors. He concludes that assessment uniformity increases with frequent reassessment by reporting that an additional year of reassessment lag may lead to 1.6 percent reduction in assessment uniformity and an additional reassessment over the previous four years improves assessment uniformity by 17.8 percent. While Eom(2008) was the first paper to explicitly address the potential endogeneity in reassessment lag and frequency, there are various limitations of using cross-sectional data of a single year to estimate dynamic reassessment behaviors. Echoing Mikesell and Eom's findings, we test whether reassessment lag and frequency as well as actual commitment to specific cycles improve uniformity in assessment.

3. Evolution and Status of Practice in Virginia and New York State

3.1. Property Assessment in Virginia

The Commonwealth of Virginia (stated as the Virginia State from below) is a representative case of a strong Dillon Rule state where local governments are bound by various institutional constraints. The institutional setting in Virginia provide an opportunity to conduct a natural experiment on assessing jurisdictions to test the effect of their assessment behavior on assessment outcomes. In Virginia, 134 counties and independent cities are the tax assessing jurisdictions, and function under the supervision of the local Commissioner of Revenue.[77] Counties and independent cities in Virginia conduct revaluation according to a regular cycle and the State Department of Taxation monitors each locality's assessment practice through an annual sales-ratio study. The State annually surveys all assessing jurisdictions to monitor whether they conduct mass reappraisal according to the stipulated cycle, perform physical inspection, elect or appoint full time assessor, and contract out reappraisal. The sales ratio study also reports the median assessment ratio as well as horizontal and vertical equity measures including COD and price related differential ratio based on randomly sampled market transactions of properties across the state. The State law also requires local assessing jurisdictions to reassess all residential properties at 100% of full market value, which contrasts with the case of NYS that allows fractional assessment.

In 1984, the Commonwealth of Virginia enacted a tax code that mandated all local assessing jurisdictions to adopt regular reassessment cycles, that set the maximum limits of interval periods between years of mass reappraisal. Section 58. 1-3252 of the tax code requires counties to conduct mass reappraisal every four years with the exception of smaller counties that can elect their own cycle at either five or six-year intervals. Meanwhile, independent cities with

[77] Counties are also the primary local level of general purpose government in Virginia, providing services for public education and health services. Unlike in New York, school districts are operated at the county level.

population above 30,000 are required to reassess every two years, while smaller cities with population of 30,000 or less may elect a four-year cycle (section 57. 1-3250, ibid).

For the counties and cities that never changed their reassessment cycle, the *timing* of reassessment that occurs decades after the enactment of the 1984 tax code is plausibly exogenous to the change in assessment outcomes during later periods. Since the cycle was pre-determined in 1984, there is no exact same combination of assessing jurisdictions conducting revaluation in a given year. While most counties and cities did not change their reassessment cycle since 1984, the tax code does not hinder the local jurisdictions from reassessing more frequently than the pre-determined cycle. Between 2002 and 2016, 36 counties and cities elected a shorter cycle than previous years, and 17 reverted to a longer cycle. This suggests that the local jurisdiction's decision to maintain the pre-determined cycle or change the cycle to either a shorter or longer cycle can be potentially endogenous to assessment outcomes. In order to address the potential threats to internal validity from reverse causality of assessing jurisdictions adjusting their reassessment cycle due to concern of high COD or high tax rates, we employ instrumental variables which will be specified in the following section.

3.2. Property Assessment in New York

Unlike most states, New York is one of the seven that do not mandate regular assessment cycles (two others, New Jersey and California, reassess upon sale or improvement)[78]. Instead, NYS uses financial aid as an incentive for localities to comply. The absence of regular reassessment has led to deterioration in the uniformity of property valuation, eroding the equity

[78] A northeast neighbor, Massachusetts, simply orders its localities with faulty assessment rolls to conduct reassessments; in cases of non-compliance, the state may hire a contractor for the job and charge the locality for the project (2010 MVS).

basis of the whole property tax system (NYS ORPTS, 2010).[79] NYS allows its tax assessing municipalities full discretion to set their own cycle or absence thereof, of assessment. Tax assessing jurisdictions in New York are not only cities (62), but mostly towns (932), and until 1983 all villages (553), totaling over 1,500[80]. According to the NYS ORPTS annual report, the number of assessing units stood at 1,029 as of 2016 (61 cities, 2 counties, 930 towns, and 127 villages and CAP units). Since most of these assessing units are small and many rural, the cost and data scarcity issues discussed above are big hurdles for them to assume short reassessment cycles. On the cost side, for example, 524 jurisdictions share sole assessors with other assessing units, put another way, around 180 assessors serve in multi-jurisdictions.

A report from the NYS ORPTS states a strong assumption that was used for reviewing local assessment practices until 1998: Their report states that "uniformity exists for three years following a state-approved reassessment" (NYORPTS *1998*), though they also emphasized that "frequent reassessment contributes strongly to uniformity and equity" (ibid, p.7). This is typical of the cyclical assessment approach under which assessed values are held constant till the next reassessment. During years of volatile markets and high inflation, property values do change a lot and transactions of property (sales) cause equity issues (though assessing units can ameliorate on new houses by valuing them back to the date of the last assessment).

New York's property tax system has also long remained at a lower-local level in comparison to many other states. As recently as 1983, all its 553 villages were separate tax assessing units beside 1,546 towns and cities (NYORPTS 2001). By 2010, only less than one-

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[79] Though property tax administration is decentralized in New York, the State's Department of Taxation and Finance is charged by state law to regularly monitor the equity of assessments. As required by Section 1200 of NYS Real Property Tax Law, the Office of Real Property Tax Services in the Department has, since the 1980s, been conducting annual market value survey of real properties in the state, thereby analyzing assessment uniformity (2010 MVS, p. 1).

[80] In contrast, Georgia and California have counties as tax assessing units; thus, their units are few and much larger.

third (127 out of 553) of villages still had assessors, for village purposes, property assessment had become a town and city function and the total number of assessing jurisdictions was reduced to 1,029. NY State promoted more frequent reassessments of local assessing units, not by statutory mandate but by providing financial and technical assistance, starting in the late 1970s, the aid programs have continued, though in varied forms. Over roughly a quarter century, the state made strides in changing tax administration. From 1985 to 2000, most units received reassessment aid and conducted reappraisals according to their submitted plans, whereas one-fifth (192) did not reassess a single time (NYORPTS 2001). Thus, the NYS aid programs for reassessment offer opportunities for semi-experiments in this study.

New York State first introduced Attainment Aid Program in 1977 to encourage local tax assessing units that have not updated their tax assessment roll for a long period to conduct revaluation. This program was reinforced in 1991 and changed to Maintenance Aid program that provided $5 per parcel in the year of reassessment and $2 per parcel in subsequent years, if the jurisdictions met the state standards of reassessment quality. The payments were only provided once in any three-year period as long as the jurisdiction did not receive any reassessment aid within the previous three years. This restriction changed in 1999 (by the enactment of Chapter 405 of the 1999 Laws), when the state replaced the Maintenance Aid program (fully replaced by 2001) with a new Annual Reassessment Aid program. In addition, the state also provided triennial aid for localities that conducted recent reassessments but are not able to reassess annually. From then on eligible applicants were provided $5 per parcel on each roll in the year they annually reassessed. While the provision was to expire after 2004, and the payments were reduced to $3 per parcel, the state eventually extended the provision of benefits until 2009 For those who were eligible for Triennial Aid, the state paid up to $5 per parcel upon completion of full revaluation. The state removed sunset and phase down provisions under Chapter 655 of the 2004 laws to encourage participation. Jurisdictions were also still eligible for annual aid even after receiving triennial aid. By 2007, 250 towns and cities were reassessing annually and by 2010, 27% of all assessing units were conducting cyclical reassessment plans to the state.

The incentives changed in 2009 when the state cut real property tax-related appropriations by 12.5% (under Chapter 502 of the laws of 2009). By 2009, the State Operations part of the ORPS budget was $32.56 million, $13.965 of which were paid to municipalities in the form of intergovernmental grant. The state terminated the previous Annual and Triennial Aid programs in 2010, replacing it with Aid for Cyclical Reassessments (ACR) program that is more restrictive in terms of its eligibility requirements. The constant increase in participation in aid programs and reassessment activities between 2000 and 2009 suddenly declined after this policy enactment, as illustrated in Figure 1. After the 2010 reform, cyclical reassessment aid was only payable to assessing units conducting reassessment according to their submitted plan (minimum every four years) that did not benefit from the aid program in the previous two years. Also, municipalities were required to remit all aid payments received since the last reappraisal, once they discontinue a planned reappraisal.

4. Data and Methodology

4.1. Sample and Data

In this study, we use two separate state samples. With the Virginia sample, we analyze the relation between reassessment lag and assessment uniformity. Next, in the analyses using the New York sample, we study the relation between the length of reassessment cycle and assessment uniformity. In the final analysis using a subsample of NYS assessing units, we test the effect of annual reassessment on assessment quality.

Dependent variables

Our main dependent variable is coefficient of dispersion (COD) which is a commonly used performance measure of assessment uniformity in the public financial management literature (Geraci and Plourde 1976; Bowman and Butcher 1986; and Chicoine and Giertz 1988). We normalize COD in a negative logged form, for a number of reasons. First, the distribution of COD is highly skewed towards zero. Since relative differences in COD are more useful than absolute distances, we use the natural logarithm of COD. The log form gives more weight to changes at low values than at high values, which Bowman and Butcher (1986) argue is more preferable. We then multiply ln(COD) by negative one for the ease of interpretation – a positive coefficient on a point estimate would indicate improvement in horizontal equity or assessment uniformity.

The New York state Office of Real Property Tax Services (ORPTS) only reports COD for a sample of assessing units that have not conduced revaluation over the past three years prior to the market value survey year. For this study, we use the annual sales data to calculate an annual COD for each assessing unit. We restrict our parcel level data to arms-length sales so as to exclude outliers from anomaly. Then we focus on three major sub-classes of residential property, which are one-family year-round residence, rural residence with acreage, and two-family year-round residence. Together, these three classes account for 67% percentage of the data and are the most representative sub-classes. From these three classes, we develop two measures for use in the tests, one is of all three classes and the other is only of single-family year-round residences.

We focus on assessment uniformity of residential properties for three reasons. IAAO (2010 Standard on Ratio Studies) sets more stringent CODs for residential property. Some of the IAAO standards for acceptable COD ranges related to this study are as follows. Single-family housing: 5 to 10 in new or homogeneous areas, 5 to 15 in older or heterogeneous areas. Other types of residents like rural, seasonal, recreational, manufactured (2-4 units), 5 to 20. Vacant land: 5 to 25. CODs lower than 5 may be due to sales chasing or non-representative sampling (IAAO 2010). The results from assessing units sampled in New York's *Market Value Survey* series show that the percent of jurisdictions achieving acceptable uniformity is systematically lower on residential properties than on all properties at all population density levels.

One big challenge in estimating COD arises from extremely low sale prices or assessed values. In cases where the sale price of a property is abnormally below the assessed value, we assume that this may not be a problem of tax administration, but rather the (owner) seller's credit problem due from, for example, bankruptcy-triggered auction (though such cases were rare in the dataset). Cases of extremely low assessed value arise mainly, if not solely, from outdated assessments in the jurisdiction. In order to cope with such outliers, we calculate assessment ratio per parcel and winsorize extreme values of assessment ratio. To winsorize is to replace outliers larger than (or less than) the highest (or lowest) one percentile with those minimum (or maximum) values. Then we identify the median assessment ratio per municipality and calculate the absolute deviation per parcel from the municipality's median. Finally, we divide the average deviation at the municipality level by the municipality's median assessment ratio and multiply the value by 100 to get a measure of COD in percentage format. The equation to calculate COD is the following:

$$COD_m = \frac{100}{R_m}\left(\frac{\sum_1^i \ |R_i - R_m|}{N}\right),$$

where $|R_i - R_m|$ is the absolute deviation from the median per parcel, N is the number of parcels sampled in the sales data, R_i is the assessment ratio for each parcel in the sales data, and R_m is the median assessment ratio.

In the Virginia sample, we use COD measured by the Commonwealth of Virginia's Department of Taxation. Virginia's COD measures are based on randomly sampled fair market transactions from the Virginia Sales Price Ratio Studies conducted annually by the Virginia Department of Taxation. We use county and independent city level COD to construct the dependent variable for the Virginia sample.

Independent variables

We primarily use administrative and survey data on local reassessment activities of counties and independent cities in Virginia as well as towns and cities in New York. We obtain data from various sources for the New York sample – local financial data published by the New York Comptroller's Office, population from the U.S. Census, and *Market Value Survey* by the New York Office of Real Property Tax Services (ORPTS, via Freedom of Information Act request). Year and frequency of reassessment are also reported by the New York ORPTS.

The key variable of interest in the Virginia model is a measure of reassessment lag, operationalized as years since last reassessment. This variable ranges from 0 to 6 since the maximum reassessment cycle of counties and cities in Virginia is stipulated as 6 years, by the state law. We also include interaction terms between the reassessment lag variable and dummy indicators of each cycle (five different dummy variables of each cycle from every 2 years to every 6 years) to the baseline model in some of the specifications. In the New York model, the key independent variable is a measure of reassessment cycle in addition to reassessment lag and the same set of interactions as in Virginia model. There is larger variation in cycle across time among assessing jurisdictions in New York, as opposed to the Virginia's case where most counties and independent cities did not change their cycle since 1984. This institutional setting allows us to measure the effect of changes in the cycle *per se*, which is the underlying question of interest, in addition to reassessment lag that measures how recently reassessment was conducted last time within a given cycle.

We also use a rich set of jurisdiction-level characteristics related to reassessment behaviors and local environment that vary over time as covariates. These includes 1) logged total assessed value of exempt parcels, 2) logged value of total municipality budget minus assessment cost, and 3) total parcel count. We control for average assessment ratio as well as methods of reassessment such as use of computer assisted mass appraisal (CAMA) and actual reappraisal. We also control for reassessment aid (logged) received per municipality each year and the shares of different property classes to account for the heterogeneity in the structure of the local economy. We use assessing unit's aggregate average of parcel-level characteristics such as acreage of lot and indicators of properties that sold over one million USD.

Indicators of year of reassessment, physical inspection, full-time assessor, contract versus in-house inspection as well as median sales price, median assessment ratio, coefficient of dispersion and share of exempt properties or properties by class are all provided by the Virginia Department of Taxation. Budget data on property tax levy, intergovernmental grant and total revenue are provided by the Virginia Auditor of Public Accounts.

Other socio-economic covariates including county level unemployment rate is from the Bureau of Labor Statistics. Demographic covariates are also included to account for the potential confounding effects of local voters' time-varying characteristics. These covariates include the share of female, Hispanic, black and non-hispanic white population, population of children between age of 0 and 9, share of population over age of 65. All population estimates are provided by the U.S. Census.

Instrumental variables are also constructed using housing price index from multiple sources. The source median housing price of all sales and top tier sales for Virginia counties and cities is from Home Value Index provided by Zillow. An alternative measure of median housing price is the Federal Housing Agency's Housing Price Index based on mortgage data. Growth rate in median market value of sold properties, housing dispersion ratio data at towns and cities in New York are calculated by using parcel-level NY Market Value Survey data.

Our final Virginia sample consists of 133 unique assessing units in Virginia while the New York sample is comprised of 43 Cities and 779 Towns. The sample period runs from 2000 to 2016 for the NY sample and from 2002 to 2016 for Virginia sample. Table 2 provides a summary of data sources. Panel A in Tables 3 and 4 provides the summary statistics of observable characteristics of various groups of assessing jurisdictions in each state that had different maximum length of reassessment cycle. Panel B in Table 3 compares the pre and post difference in variables used in the analyses among three groups of assessing jurisdictions in Virginia. The first two columns

4.2. Identification Strategies

Effect of Lag and Length of Reassessment on Assessment Uniformity

The key challenge in isolating the effects of reassessment lag or cycle on assessment uniformity is that the direction of the causal relation can go both ways. Changes in reassessment lag or cycle may lead to changes in actual assessment uniformity, while an assessing jurisdiction's decision to reassess in a given year may also be a function of its prior year's (i.e., current) COD, which is a typical performance measure of property assessment. In order to cope with potential bias in the estimates from reverse causality, we first employ a control function approach. As an alternative approach, which is our preferred identification strategy, we instrument for the reassessment lag and cycle variables with multiple instrumental variables. We use various measures of dispersion and the growth in housing price to construct instrumental variables separately for samples of assessing jurisdictions in Virginia and New York, respectively. The outcome production function equation and the treatment equation can be summarized as follows:

$$Y_{it} = f_Y(R_{it}, \boldsymbol{V}_{it}, \boldsymbol{X}_{it}, \delta_i, \theta_t, \varepsilon_{it}) \qquad (1)$$

$$R_{it} = f_P(\boldsymbol{Z}_{it}, \boldsymbol{X}_{\text{it}}, \backslash\delta_i, \theta_t, \epsilon_{it}) \qquad (2)$$

where Y_{it} denotes the dependent variable, $-\ln(COD_{it})$. $\boldsymbol{V}_{it}$ is a vector of other determinants of COD that are endogenous in the case of New York (A_{ict} and L_{ict} each indicating state aid for reassessment and level of assessment), but exogenous in the case of Virginia so included as part of $\boldsymbol{X}_{it}$. ε_{it} is the unobserved residual from the outcome equation and R_{it} is a measure of reassessment lag or the length of reassessment cycle in a given year *t*. $\boldsymbol{Z}_{it}$ is a vector of instrument variables for jurisdiction's decision to reassess in a given year which will be elaborated below.

The exogenous covariates in vector $\boldsymbol{X}_{ict}$ includes share of exempt properties out of total number of parcels, share of commercial parcels, share of industrial parcels, median assessment ratio as well as dummy indicators of physical inspection, full-time assessor, whether assessment is contracted out. The models also include time varying demographic characteristics that may capture the confounding effects of median voters' preference for reassessment such as the share of female, Hispanic, black and non-hispanic white population, population of children between age of 0 and 9, share of population over age of 65.

Three instrumental variables from different sources are used for the endogenous reassessment lag variable in the Virginia model. We are particularly concerned of potential reverse causality, i.e. assessing units with shorter intervals between reassessment periods might reassess more frequently than other assessing jurisdictions due in part to high demands for uniform assessment. Also, assessing units with less frequent reassessment might choose to have longer reassessment cycle or longer intervals in response to high resistance from owners of expensive properties that would not benefit from updating property valuation. The first instrument is housing price dispersion (HD_{mt})of is constructed using Zillow's data and measured as annual growth in the ratio of median housing price in the top 30% divided by median housing price of all properties sold (One of the major downside of using Zillow data is that we lose 54 assessing jurisdictions, most of which being independent cities, from the Virginia sample, because they do not have enough records of sales to separately report median market value across the distribution of housing price. The second instrument is growth in median housing price using the Federal Housing Agency's Housing price index based on mortgage data. The third instrument is the growth rate in median market value of sold properties, using data from the Virginia Department of Taxation.

The underlying intuition of using these instruments is that the owners of more expensive properties that expect to gain from less frequent revaluation may impose political pressure against frequent reassessment. Their potential gain may depend on the growth in the market value of their houses, relative to the growth in values of other houses. Property values tend to rise faster where income is higher, or expensive properties tend to appreciate at a higher rate than cheaper properties (conditioning on infrequent reassessment, which is correlated with local wealth). Eom (2008) proposed using similar measures of distribution houses and changes of house value dispersion over time as potential instruments for reassessment activities. However, he was not able to use them as valid instruments for town/city level assessment activities in his cross-sectional analysis and eventually used county average values of reassessment variables as instruments. Our underlying assumption of independence can be expressed as (3) which implies the exclusion restriction condition (4):

$$\boldsymbol{Z}_{it} \perp \varepsilon_{it} \mid \boldsymbol{X}_{\mathrm{it}} \qquad (3)$$

$$\boldsymbol{Z}_{it} \perp Y(r)_{\mathrm{it}} \mid \boldsymbol{X}_{\mathrm{it}} \text{ for all r} \in supp(R_{it}) \qquad (4)$$

In the New York analysis, we use similar instruments for reassessment lag and cycle variables. We use different combinations of four instrumental variables. The first instrument $\mathrm{z}_{1,\mathrm{ict}}$ is the growth rate in median market value of all sold properties. The second and third instruments are town and city level average measures of housing value dispersion constructed from parcel level sales data. The second one, $\mathrm{z}_{2,\mathrm{ict}}$, is the annual growth rate of the median housing price in the top quartile relative to the bottom quartile. The third measure, $\mathrm{z}_{3,\mathrm{ict}}$, is the annual growth in the ratio of dispersion in market value from the jurisdiction median in the top quartile, relative to the bottom quartile. The fourth instrument, $\mathrm{z}_{4,\mathrm{ict}}$ which is used for robustness check is similar to Eom (2008)'s measure, which is the average reassessment cycle of neighboring jurisdictions within the same county.

Following the Duncombe & Yinger (2007) that builds upon the copy cat or yardstick theory to construct instruments, we use the characteristics of neighboring jurisdictions as instruments for A_{ict} and L_{ict}. The assumption of this diffusion approach is that each jurisdiction is influenced by decisions of similar comparable jurisdiction's decisions that are in the same labor market area (counties, in New York's case) and treat the neighbor' receipt of state aid and level of assessment as exogenous to assessment uniformity in jurisdiction i at time t. The final instruments for A_{ict} and L_{ict} are the average amount of reassessment state aid received ($\mathrm{z}_{\mathrm{a,ict}}$) and median assessment ratio among other jurisdictions within the same county ($\mathrm{z}_{l,\mathrm{ict}}$), respectively.

$$\mathrm{z}_{\mathrm{a,ict}} = \sum_{k \neq i} A_{kct} | \; k, i \text{ in the same county}, c$$

$$\mathrm{z}_{l,\mathrm{ict}} = \sum_{k \neq i} L_{kct} | \; k, i \text{ in the same county}, c$$

Equation (5) is the baseline reduced form model including jurisdiction and year fixed effect. In order to address the potential bias in $\pi_1$by treating R_{it} as an exogenous variable, we employ control function and 2SLS estimators.

$$Y_{it} = \pi_0 + \pi_1 R_{it} + \Gamma \boldsymbol{V}_{it} + \Upsilon \boldsymbol{X}_{it} + \delta_i + \theta_t + \varepsilon_{it} \quad (5)$$

To begin with control function approach, we run the following first stage regressions by regressing each of the endogenous variables on all exogenous variables. After retrieving the residuals from each regression ($\epsilon_{1,ict}$, $\epsilon_{2,ict}$ and $\epsilon_{3,ict}$), we include them as additional regressors to baseline regression (5).

$$R_{it} = \pi_0 + \pi_1 \boldsymbol{Z}_{it} + \Upsilon \boldsymbol{X}_{it} + \delta_i + \theta_t + \epsilon_{1,ict} \quad (6)$$

$$A_{it} = \pi_0 + \pi_1 \mathrm{z}_{\mathrm{a,ict}} + \Upsilon \boldsymbol{X}_{it} + \delta_i + \theta_t + \epsilon_{2,ict} \quad (7)$$

$$L_{it} = \pi_0 + \pi_1 \mathrm{z}_{\mathrm{l,ict}} + \Upsilon \boldsymbol{X}_{it} + \delta_i + \theta_t + \epsilon_{3,ict} \quad (8)$$

The final model to be estimated by 2SLS is also an extension of equation (5) but the endogenous variables (R_{it}, A_{it} and L_{it}) are instrumented by exogenous variables ($\boldsymbol{Z}_{it}$, $\mathrm{z}_{\mathrm{a,ict}}$ and $\mathrm{z}_{\mathrm{l,ict}}$). All instruments pass the tests for weak and valid instruments. The first stage Cragg-Donald F statistics are all above the critical values from Stock & Yogo (2005) and reported in Tables 4, 6 and 7 along with the p value of Hansen J statistics. In all models, we fail to reject the null hypothesis of excluded instruments being uncorrelated with the error term. The standard errors are also clustered at the jurisdiction level and we also weight our estimates by the original parcel count of each jurisdiction.

Effect of Annual Reassessment on Assessment Uniformity

The main empirical question raised in the previous section is whether reassessment frequency measured by the time lag between reassessment period and the length of reassessment cycle affects assessment outcomes. In this section, we narrow our target population to a group of assessing units that conduct annual reassessment. The majority of tax assessing jurisdictions in New York conduct triennial rather than annual reassessment, largely due to the high administrative cost (Bick 2016; NYAA 2018). Therefore, it is important to understand whether jurisdictions that commit to annual reassessment achieve better assessment uniformity than comparable jurisdictions that do not.

In this analysis, we test whether annual reassessment improve horizontal equity and affect effective property tax rates. Hypothetically, those jurisdictions that reassess annually should have better horizontal equity (lower cod). Given the high initial cost of conducting full reassessment, whether the benefit from frequent reassessment (equity, efficiency, adequacy and transparency) is large enough to balance the cost is an important empirical question for local policy makers. We use event study framework to address this question.

We test whether annual reassessment for three (or four in a separate analysis) consecutive years lead to any change in assessment uniformity and tax rate. The treated group in this analysis are those that started to reassess between 2003 and 2008 for three (or four) consecutive years then stopped reassessing. In order to cope with potential selection bias in our estimates, we need to select a comparable comparison group. We compare the outcome of this group to that of a comparison group that do not annually reassess until the end of the post period: The comparison group start consecutive annual reassessment at least from six years after the early adopters started annual reassessment.

To address these two questions, we use semi-parametric event study framework with inverse probability weighting. We use the event study framework to see the timing of changes in the outcomes relative to the timing of initiation of annual reassessment. This approach has the flexibility to allow the effect of annual reassessment on assessment outcomes to vary relative to the date of initiation of reassessment. The baseline non-parametric event study model can be specified as follows:

$$Y_{it} = \alpha + \rho D_i + \sum_{j \neq -1} \beta^p D_{it}^p + \boldsymbol{X}_{it} \Gamma + \delta_i + \theta_t + \varepsilon_{it} \quad (9)$$

where D_{it}^{p} is an indicator of lead and lag from the year of first reassessment for each jurisdiction i in year t. p is an index of periods relative to the start of annual reassessment and period 0 indicates the first year when annual reassessment began. The omitted category for D_{it}^{p} is one calendar year before the starting year of consecutive annual reassessments. Coefficients of D_i, ρ is the difference in COD between the two groups of municipalities in *t-1* period. The estimated coefficients on the lagged indicators (β^p) are the differential change in Y_{it} between the municipalities that started reassessment and those that did not relative to period -1. We use the same model to capture any improvements in the outcomes among municipalities that conduct annual reassessment for 3 to 7 consecutive years, respectively. All models include municipality fixed effects as well as group specific time trend. We control for a vector of municipality level observable characteristics that vary across time, $\boldsymbol{X}_{it}$ that includes reassessment frequency since 2000, average assessment ratio, log of reassessment budget, share of each property class, years since first assessment, total parcel count and state reassessment aid. We also control for receipt of state reassessment aid in the first analysis of estimating the effect of conducting annual reassessment.

To cope with potential endogeneity of municipalities' decision to reassess each year, we impose weights to the control units using propensity score based inverse weighting strategy to seek balance between two groups of municipalities. This allows control units that seem to have higher probability of receiving treatment to receive larger weight. Following Imbens (2000) and Hirano, Imbens and Ridder's (2003) approach, we calculate the following weight,

$$w_i = T_i + \frac{\hat{e}(X_i)}{1-\hat{e}(X_i)}(1 - T_i)$$

where $\hat{e}_i(X_i)$ is the estimated propensity score for a municipality. $e(X_i) = \Pr(T_i = 1|X_i)$.

w_i eventually becomes 1 for treated group and $w_i = \frac{\hat{e}(X_i)}{1-\hat{e}(X_i)}$ for the control group. As long as the balancing assumption is met as in equation (3),

$$\mathbb{E}\left\{D_i X_i - \frac{\hat{e}(X_i)(1-D_i)X_i}{1-\hat{e}(X_i)}\right\} = 0$$

we can estimate the average treatment effect on the treated as the following;

$$(Y_{i=1,t} - Y_{i=1,t=0}) - (\sum_{i=2}^{I+1} Y_{it} - \sum_{i=2}^{I+1} w_i \, Y_{i,t=0})$$

where $Y_{i=1,t}$ is the outcome for a treated municipality (i = 1) that is committed to consecutive annual reassessments in year t, and $Y_{i=1,t=0}$ is outcome for that municipality in one year prior. Panel B in Table 4 provides the summary statistics of observable characteristics among both the treated and the control units in NY that ever committed to annual reassessment.

5. Results

5.1. Effect of Reassessment Lag and Length of Cycle on Assessment Uniformity

Figure 2 illustrates the distribution of maximum length of reassessment cycle among VA assessing units as well as the negative association between the length of maximum cycle and assessment uniformity. Figure 3 depicts the stylized facts about the relation between reassessment lag and assessment uniformity using VA sample. The histogram in Panel A in Figure 3 plots the distribution of reassessment lag among all assessing jurisdictions in VA. Panel B uses a subsample that excludes jurisdictions that conduct annual reassessment, which is the reference group. The descriptive figures suggest a negative association between reassessment lag and assessment uniformity: The longer the interval between two periods of revaluation, the worse the assessment quality becomes. Moreover, the negative relation shows a linear pattern.

The baseline fixed effect model estimates reported in the first two columns in Table 5 supports the descriptive observation that an additional year of no reassessment is associated with deterioration in assessment uniformity. This finding also holds when addressing the potential endogeneity in three endogenous variables with the control function approach: The coefficient on reassessment lag variables remain statistically significant when using various combinations of three exogenous variables ($z_{1,ict}$, $z_{2,ict}$ and $z_{3,ict}$) based on home value dispersion and growth in median market value. When including the interaction terms, we find evidence of significant decrease in assessment uniformity by 4 to 5 percent among jurisdictions that conduct reassessment biannually and every four years per additional year of skipping reassessment.

Tables 5 and 6 suggest significant negative effects of reassessment lag on assessment uniformity among assessing jurisdictions in Virginia. The signs on estimates are consistent across different specifications when using both control function and instrumental variables. Particularly the IV results show more pronounced negative effects among jurisdictions that reassess every other year. While the average deterioration in assessment uniformity for an additional year of no reassessment is a little over 2 percent for all groups, those that reassess biannually are shown to experience a 9.5 to 10.8 percent decline in assessment quality. Assessment uniformity also seem to deteriorate by over 3 percent among jurisdictions that reassess every four years per each additional interval year.

The control function and IV estimates reported in Table 7 suggest that a longer reassessment cycle leads to deterioration in assessment uniformity among assessing jurisdictions in New York. The IV estimates suggest that an increase in the length of reassessment cycle by one year leads to deterioration in assessment uniformity by approximately 4%, relative to municipalities that conduct annual assessment. On the other hand, the coefficient estimates on the interacted terms tells us a slightly more complex story. The average effect seems to mask the hidden heterogeneity across different groups of jurisdictions that have various cycles. In fact, municipalities that conduct reassessment biannually or triennially show nonnegative change in assessment uniformity during interval periods. This indirectly lead us to question the necessity of requiring annual reassessment as the norm cycle, considering the high cost and administrative challenges for conducting mass reappraisal every year. Nonetheless, when we trim our sample by excluding jurisdictions that have longer maximum cycle than 15 years, we find suggestive albeit weak and inconsistent evidence of negative association between reassessment lag and assessment uniformity. An additional year of reassessment lag may contribute to a decrease in assessment uniformity by approximately 1% on average, and by 2% for jurisdictions that reassess every four years. On the other hand, reassessment lag shows to have positive effect on assessment uniformity among jurisdictions that reassess triennially as found in the full sample.

5.2. Event Study Estimates from New York

A closely related question raised in this study is whether annual reassessment effectively leads to improvement in assessment uniformity. Our event study result provides suggestive evidence of consecutive annual reassessments improving assessment uniformity. Figure 4 and columns (1) and (2) in Table 10 show the results for assessing jurisdictions that reassessed for three consecutive years in New York. The comparison group are late adopters or jurisdictions that started consecutive reassessments after the end of the post period. Each coefficient on D^j measures the differential change in outcomes between two groups, relative to the reference period, which is one year before the treated group initiated consecutive reassessments. The insignificant pre period estimates reveal that all the outcomes trended similarly among both early and late adopters before the early adopters started reassessing. Jurisdictions that committed to consecutive annual reassessments tend to observe improvement in horizontal equity as measured by coefficient β^3 of dispersion for single-family residence properties, after they had conducted three annual reassessments.

Such trend is evident among those who committed to four consecutive years of annual reassessment, as reported in the last two columns in Table 10. The positive estimate of β^4 in the three residential sub-classes model suggest that assessment uniformity improved significantly by 42 percent after three consecutive reassessments, relative to the year before they initiated such commitment. The magnitude of this effect shows to be a considerable improvement, considering the fact that the assessment uniformity for single family residential properties was higher among treated units than the comparison group, one year before start of consecutive reassessment (although the difference is not statistically significant). This suggests that the jurisdictions that committed to four years of annual reassessment observed improvement in their horizontal equity, relative to late adopters who did not reassess during the same period.

6. Conclusion

In this study, we have examined the effect of assessment frequency on assessment performance using natural experiments for two case studies of assessing jurisdictions in Virginia and New York. As a strong Dillon state, the Commonwealth of Virginia mandated a fixed assessment cycle in the 1984 and local assessing units have little autonomy in administrating property assessment. On the other hand, New York State is a strong home rule state employing financial incentives and no mandates for property assessment cycle. We tested the effect of assessment frequency on horizontal and vertical equity in property assessment, by employing multiple instrumental variables to account for the potential endogneity in the decision to reassess across assessing units in two states. We also conduct a semi parametric event study analysis using parcel-level sales data and detailed jurisdiction-level administrative data on assessment activities in New York.

This research yields several suggestive findings. First, we find evidence that an additional year of reassessment lag has negative effect on assessment uniformity, that suggests infrequent reassessment may undermine fair assessment. Specifically, we find an average 2 percent decline in assessment uniformity per additional interval year between mass reappraisal among assessing jurisdictions in Virginia.

Second, we find that the length of reassessment cycles may also contribute to assessment uniformity. Using control function and instrumental variables we find that longer reassessment cycle leads to deterioration in assessment uniformity among towns and cities in New York. Our event study estimates using sub-sample of assessing jurisdictions that ever committed to annual reassessment for consecutive multiple years also suggest that assessing jurisdictions that annually reassess experience improvement in assessment uniformity, relative to comparable jurisdictions that did not. We also observe improvement in assessment uniformity typically from the year the jurisdictions complete their annual consecutive reassessment.

Our findings have limited policy implications to be extrapolated to tax assessing jurisdictions in other states with different institutional settings. We further plan to alternative approaches by using geographic boundary discontinuity and parcel level data to better assess whether initiation of annual reassessment lead to improvement in outcomes. We can extend our analysis at the micro level to assess whether the cycle has any bearings on tax burden, controlling for various parcel level characteristics. We can also use additional measures of outcomes such as price-related-differential (PRD) to assess the effect of reassessment cycle on vertical equity. More importantly, we hope to test the efficiency and equity effects of shifting from annual cycle to a less frequent but regular cycle such as triennial cycle in our further analysis.

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Table 1. Reassessment cycle of assessing jurisdictions by state

Required Reassessment Frequency	# of states	List of states
Annual	10	Alaska, Arizona, Georgia, Massachusetts, Michigan, Montana, Nebraska, North Dakota, Pennsylvania, West Virginia
Every 2 years	5	Colorado, Iowa, Missouri, New Mexico, Virginia cities
Every 3 years	5	Alabama, Arkansas, North Carolina(counties with 75 thousand or more population), Texas, D.C.
Every 4 years	8	Illinois*, Louisiana, Maine, Minnesota, Oklahoma, Virginia counties, Washington, Wyoming
Every 5 years	7	Florida, Idaho, Nevada, South Carolina, Utah, Wisconsin
Every 6 years	2	Ohio, Tennessee
Every 10 years	2	Connecticut, Rhode Island
Varied requirements by locality	3	Virginia, North Carolina, Kansas
Upon sale or improvement	2	New Jersey, California
No requirements	7	Delaware, Hawaii, Mississippi, New Hampshire, New York, Oregon, South Dakota

Source: Justin Higginbottom (2010), "State Provisions for Property Reassessment," Tax Foundation, *Fiscal Fact* No. 223, April 29, 2010. *Note** Cook County as an exception in Illinois, where properties are reassessed triennially.

Table 2. Variables and source of data

Variables	Source
Virginia Sample	
Budget and property tax levy	Virginia Auditor of Public Accounts
Coefficient of dispersion	Virginia Department of Taxation, Virginia Annual sales ratio studies
Indicator of full time assessor	
Effective property tax rates	
Reassessment frequency	
Median assessment ratio	
Median sales price	(same as above)
Share of exempt properties	
Share of property classes	
Indicator of contract versus in-house inspection	
Cycle of physical inspection	
Median housing price of all sales and top tier	Zillow sales data
New York Sample	
Assessment Budget	Comptroller Office, Open book
Assessment operation	Office of Real Property Tax Services
Real property tax levy	
State aid for real property tax	
State Equalization rate	
Log assessed value of exempt properties	(same as above)
Property tax levy, nominal rate	
Reassessment activity(frequency)	
Parcel count by class	
Share of property classes	
Coefficient of dispersion (authors' calculation)	NY Market Value Survey
Assessment method(CAMA, appraise)	
Share of million dollar MV properties	(same as above)
Number of sales of single family houses	
Acre size of properties	
Common Covariates	
Unemployment rate	Bureau of Labor statistics
Share of gender, race and age groups	U.S. Census
Median Housing Price Index	Federal Housing Agency

Table 3. Summary statistics: Virginia sample

Panel A. Summary statistics of VA jurisdictions by longest cycle

Variables	Annual (1)	Every 2Y (2)	Every 3Y (3)	Every 4Y (4)	Every 5Y (5)	Every 6Y (6)
Negative log of COD	-2.196	-2.574	-2.344	-2.887	-3.011	-3.228
Effective tax rate	0.96	0.85	0.93	0.64	0.58	0.48
Total parcel count	2,869	613.9	143.1	440.5	344.0	245.1
Assessment ratio	90.57	90.77	95.03	90.50	90.16	87.26
Full time assessor	1	0.911	1	0.26	0.26	0.22
Contract out (general)	0	0.088	0	0.59	0.57	0.65
Contract out (mass appraisal)	0	0.062	0	0.27	0.22	0.12
Physical inspection	0.10	0.32	0.25	0.61	0.60	0.58
Property tax/total revenue share	0.37	0.29	0.39	0.29	0.30	0.26
State &federal aid/revenue share	0.31	0.36	0.41	0.39	0.38	0.44
Appeal count	316	140	56	110	150	126
Residential (share of total)	0.96	0.94	0.97	0.91	0.91	0.84
Commercial & industrial	0.030	0.041	0.02	0.04	0.03	0.02
z_{1VA} Housing dispersion growth	-3.34	1.59	2.20	0.27	1.94	3.05
z_{2VA} HPI growth rate	4.12	2.77	3.69	2.45	3.12	2.28
z_{3VA} Growth in median MV	0.084	0.049	0.052	0.050	0.056	0.051
Population	195,249	43,027	11,903	33,108	24,537	20,333
Female (share of population)	0.51	0.52	0.50	0.51	0.50	0.49
Hispanic	0.08	0.06	0.017	0.03	0.028	0.02
Black	0.23	0.24	0.008	0.15	0.19	0.17
Non-hispanic white	0.60	0.66	0.94	0.78	0.74	0.78
Age 0 to 9 (share of population)	0.28	0.28	0.26	0.27	0.25	0.26
Age over 65	0.089	0.12	0.11	0.12	0.14	0.14
No. of jurisdictions	23	19	1	26	12	54

Panel B. Summary statistics: Assessing VA jurisdictions that changed reassessment cycles

Variables	More frequent Pre (1)	More frequent Post (2)	Less frequent Pre (3)	Less frequent Post (4)	Maintain cycle (5)
Negative log of COD	-3.03	-2.89	-3.09	-3.05	-2.79
Effective tax rate	0.55	0.61	0.64	0.55	0.71
Years since last reassessment	1.95	0.95	1.35	2.05	1.21
Total parcel count	423.8	288.5	161.3	297.8	1,032
Assessment ratio	81.37	97.49	97.52	99.94	89.40
Full time assessor	0.34	0.48	0	0.29	0.50
Contract out (general)	0.31	0.60	0.92	0.78	0.41
Contract out (mass appraisal)	0.10	0.17	0.14	0.30	0.12
Physical inspection	0.35	0.68	0.71	0.80	0.43
Property tax/total revenue share	0.28	0.32	0.42	0.37	0.28
State &federal aid/revenue share	0.40	0.38	0.30	0.36	0.39
Appeal count	38.37	242.1	2.57	149.4	170.9
z_{1VA} Housing dispersion growth	1.93	1.40	3.32	2.01	-0.40
z_{2VA} HPI growth rate	6.12	-0.62	-1.35	-0.44	2.88
z_{3VA} Growth in median MV	0.04	0.07	0.10	-0.07	0.06
Population	25,499	28,287	14,525	23,491	73,786
Female (share of population)	0.50	0.50	0.516	0.511	0.50
Hispanic	0.02	0.04	0.03	0.03	0.04
Black	0.18	0.19	0.08	0.13	0.19
Non-hispanic white	0.76	0.74	0.86	0.80	0.72
Age 0 to 9 (share of population)	0.17	0.37	0.33	0.48	0.27
Age over 65	0.13	0.11	0.13	0.12	0.12
Residential (share of total)	0.88	0.88	0.76	0.89	0.90
Commercial & industrial	0.031	0.034	0.018	0.019	0.036
No. of jurisdictions	36		17		85

Table 4. Summary statistics: New York

Panel A. Summary statistics by assessing jurisdictions with various cycles

Variables	Annual (1)	Every 2Y (2)	Every 3Y (3)	Every 4Y (4)	Every 5Y (5)	Every 6Y plus (6)
State aid for reassessment	10.30	10.04	9.826	10.08	10.27	9.152
Assessment ratio (EQR)	4.57	4.60	3.51	4.50	4.49	4.07
Share of exempt parcels	0.007	0.031	0.031	0.035	0.029	0.036
Log of assessor salary per parcel	2.74	2.42	2.69	2.54	2.71	2.49
Total parcel count	8,892	3,113	2,547	9,872	6,512	3,391
Residential (share of total)	0.758	0.623	0.569	0.668	0.668	0.627
Share of commercial	0.047	0.025	0.067	0.028	0.037	0.047
Share of industrial	0.001	0.004	0.002	0.005	0.012	0.006
Population growth	-0.001	0.001	0.0005	0.001	0.001	-0.0001
Unemployment rate	6.17	6.78	6.55	6.24	6.51	6.42
Mean tax share	2.53	1.99	1.99	2.05	1.89	1.63
z_{1NY} Growth in median MV	0.056	0.20	0.17	0.11	0.14	1.01
z_{2NY} Relative growth, median MV	-1.59	-0.05	-0.33	0.34	-0.67	-1.16
z_{3NY} Growth in dispersion	0.164	5.37	1.631	1.906	1.106	4.377
Female (share of population)	0.499	0.495	0.499	0.506	0.502	0.502
Hispanic	0.051	0.025	0.040	0.039	0.036	0.039
Black	0.077	0.036	0.043	0.078	0.058	0.047
Non-hispanic white	0.838	0.918	0.893	0.855	0.883	0.886
Age 0 to 9 (share of population)	0.248	0.226	0.254	0.236	0.239	0.237
Age over 65	0.127	0.127	0.131	0.127	0.125	0.128
Number of unique jurisdictions	3	23	13	21	14	658

Note: Each cycle refers to the longest cycle an assessing jurisdiction adopted during the sample period.

Panel B. Commitment to annual reassessment: Various treatment and control groups

Variables	3Years Treated (1)	3Years Control (2)	3Years Difference (3)	4Years Treated (4)	4Years Control (5)	4Years Difference (6)
Log (non-assessment	14.40	14.18	0.20	13.78	13.94	-0.23
budget)	(1.25)	(0.95)	(0.27)	(0.67)	(0.66)	(0.01)
Log (AV of exempt)	14.38	15.01	-0.37	14.25	14.86	-0.45
	(2.08)	(1.23)	(0.53)	(1.51)	(1.17)	(0.26)
Log (state aid)	8.60	8.94	-0.33	8.42	8.79	-0.40
	(1.22)	(0.73)	(0.50)	(1.11)	(0.61)	(0.59)
Total parcel count	2,852	2,762	0.03	1,715	2,128	-0.42
	(2,491)	(2,763)	(0.51)	(891)	(1,065)	(0.18)
CAMA	0.19	0.08	0.31	0.27	0.07	0.55
	(0.39)	(0.28)	(0.10)	(0.45)	(0.25)	(0.56)
Acre size	7.87	6.87	0.12	13.01	7.89	0.31
	(9.02)	(8.26)	(0.49)	(21.38)	(8.95)	(0.87)
Over million USD	0.13	0.22	-0.40	0.12	0.19	-0.27
	(0.20)	(0.25)	(0.09)	(0.25)	(0.25)	(0.02)
Family residential	47.24	52.04	-0.07	15.82	25.25	-0.50
	(64.21)	(72.52)	(0.24)	(16.24)	(21.07)	(0.26)
Industrial	13.81	11.04	0.13	7.76	6.48	0.17
	(22.12)	(19.96)	(0.09)	(7.44)	(7.71)	(0.04)
Appraise dummy	2.87	0.88	0.70	2.17	0.63	0.60
	(3.33)	(2.23)	(0.86)	(3.06)	(1.91)	(0.47)
Reassess year	0.07	0.31	-0.65	0.13	0.33	-0.47
	(0.25)	(0.46)	(0.40)	(0.34)	(0.47)	(0.32)
Assessment ratio	75.03	87.42	-0.50	87.83	88.41	-0.03
	(27.82)	(21.46)	(0.60)	(17.46)	(23.52)	(0.30)
No. of jurisdictions	29	54		24	54	

Table 5. Effect of reassessment lag: Fixed effect and control function estimates, Virginia

	Fixed effects		Control function			
DV: -ln(COD)	(1)	(2)	(3)	(4)	(5)	(6)
$since$	-0.030***	-0.097*	-0.051***	-0.144*	-0.028	-0.105
	(0.004)	(0.050)	(0.015)	(0.078)	(0.018)	(0.076)
$since \times every2Y$		0.037			-0.050*	-0.031
		(0.055)			(0.026)	(0.037)
$since \times every3Y$		0.088			0.017	0.007
		(0.056)			(0.029)	(0.037)
$since \times every4Y$		0.066			-0.040**	-0.041*
		(0.050)			(0.019)	(0.021)
$since \times every5Y$		0.082			0.014	0.018
		(0.051)			(0.015)	(0.017)
$since \times every6Y$		0.067			-0.014	-0.018
		(0.050)			(0.053)	(0.068)
Exogenous regressors			$\widetilde{HD}, \widetilde{MV}$	$\widetilde{HD}, \widetilde{HPI}$	$\widetilde{HD}, \widetilde{MV}$	$\widetilde{HD}, \widetilde{HPI}$
Year f.e.	Y	Y	Y	Y	Y	Y
Jurisdiction f.e.	Y	Y	Y	Y	Y	Y
Observations	1,862	1,862	1,063	1,049	1,063	1,049
R-squared	0.813	0.814	0.890	0.889	0.895	0.890

Note: N=1,862 with a sample of 133 unique municipalities for fixed effect models. N=1,063 with 79 unique counties and independent cities for models using $\widetilde{HD}$ and $\widetilde{MV}$ as exogenous determinants of reassessment frequency in the control function models. N=1,049 with 78 counties and independent cities where $\widetilde{HPI}$ is used as a key exogenous determinant of reassessment frequency. $since$ indicates years since last reassessment. The covariates include share of female, hispanic, black and non-hispanic white popualtion, share of population between age 0 and 9, population share of senior citizens, intergovernmental grant's share of total revenue, share of property tax revenue out of total revenue, population, number of property tax appeals, unemployment rate, number of total parcels, median assessment ratio, fulltime employment status of property assessor, indicator of contract-out of reappraisal service, indicator of physical inspection, % of commercial properties, % of industrial properties, total MV per parcel.

Table 6. Effect of reassessment lag: 2SLS estimates, Virginia

DV: $-\ln(COD)$	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$since$	-0.023**	-0.019	-0.022*	-0.018	-0.023**	-0.021*	-0.022*
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
$since \times every2Y$	-0.104***	-0.095**	-0.108***	-0.098**	-0.104***	-0.098**	-0.096**
	(0.038)	(0.038)	(0.039)	(0.038)	(0.038)	(0.039)	(0.038)
$since \times every3Y$	0.007	0.004	0.006	0.003	0.007	0.004	0.006
	(0.030)	(0.030)	(0.030)	(0.029)	(0.030)	(0.030)	(0.030)
$since \times every4Y$	-0.034**	-0.034**	-0.035**	-0.034**	-0.033**	-0.033**	-0.032**
	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
$since \times every5Y$	0.023	0.022	0.024	0.022	0.024	0.023	0.023
	(0.018)	(0.018)	(0.019)	(0.018)	(0.019)	(0.018)	(0.018)
$since \times every6Y$							
Excluded IV	$\widetilde{HPI}$	$\widetilde{MV}$	$HD, \widetilde{HPI}$	$HD, \widetilde{MV}$	$\widetilde{HD}, \widetilde{HPI}$	$\widetilde{HD}, \widetilde{MV}$	$HD, HPI, \widetilde{MV}$
Year f.e.	Y	Y	Y	Y	Y	Y	Y
Jurisdiction f.e.	Y	Y	Y	Y	Y	Y	Y
1st stage Fstat	29.741	30.789	25.470	26.363	25.495	22.489	22.520
Hansen J	0.1156	0.1983	0.1238	0.1847	0.1731	0.2960	0.2938
Observations	1,049	1,063	1,049	1,063	1,049	1,049	1,049

Note: N=1,063 with 79 unique counties and independent cities for models using $\widetilde{MV}$ instead of $\widetilde{HPI}$ (where N=1,049 with 78 unique counties and independent cities) as instrument for reassessment frequency. Robust standard errors clustered at county/city level reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 7. Fixed effect and control function estimates: New York (all cycles)

	OLS		Control function		IV	
DV: $-\ln(COD)$	(1)	(2)	(3)	(4)	(5)	(6)
$since$	-0.002	-0.003	0.012***	0.011**		0.077**
	(0.002)	(0.002)	(0.004)	(0.005)		(0.035)
$cycle$	-0.000	0.000	-0.016***	-0.014**	-0.040*	-0.047**
	(0.003)	(0.003)	(0.006)	(0.006)	(0.022)	(0.022)
$since \times every2Y$		0.003		0.009	0.282**	0.534**
		(0.042)		(0.043)	(0.144)	(0.222)
$since \times every3Y$		0.073***		0.078***	0.202***	0.127*
		(0.018)		(0.018)	(0.068)	(0.065)
$since \times every4Y$		-0.020*		-0.019*	0.151**	-0.004
		(0.011)		(0.011)	(0.062)	(0.039)
$since \times every5Y$		-0.007		-0.007	0.129*	0.029
		(0.013)		(0.013)	(0.068)	(0.039)
$since \times 6plusY$		0.001		0.005	0.101*	0.033
		(0.011)		(0.011)	(0.054)	(0.034)
Year f.e.	Y	Y	Y	Y	Y	Y
Jurisdiction f.e.	Y	Y	Y	Y	Y	Y
1st stage Fstat					5.787	5.591
Hansen J					0.555	0.8020
Observations	10,799	10,799	10,799	10,799	10,755	10,755

Note: N=10,755 with 864 unique municipalities. Endogenous variables that are instrumented include years since last reassessment, Cycle, interaction between dummy of each cycle and years since last reassessment, State aid for reassessment and average assessment ratio. (Added instrument: neighbors' mean frequency)

Table 8. Fixed effect and control function estimates: New York sub-sample ($longest\ cycle \leq 15$)

	OLS		Control function		IV	
DV: $-\ln(COD)$	(1)	(2)	(3)	(4)	(5)	(6)
$since$	-0.007*	-0.008*	-0.008**	-0.009*		0.011
	(0.004)	(0.004)	(0.004)	(0.005)		(0.023)
$cycle$	(0.001)	0.002	0.034	0.030	0.005	-0.001
	(0.004)	(0.004)	(0.023)	(0.023)	(0.020)	(0.020)
$since \times every2Y$		0.009		0.009	0.100	0.156
		(0.041)		(0.043)	(0.262)	(0.248)
$since \times every3Y$		0.076***		0.077***	0.013	0.027
		(0.018)		(0.019)	(0.096)	(0.081)
$since \times every4Y$		-0.017		-0.020*	-0.055	-0.047
		(0.012)		(0.012)	(0.071)	(0.054)
$since \times every5Y$		-0.009		-0.012	-0.035	-0.025
		(0.013)		(0.014)	(0.066)	(0.050)
$since \times 6plusY$		-0.003		-0.002	-0.003	0.005
		(0.012)		(0.012)	(0.059)	(0.042)
Year f.e.	Y	Y	Y	Y	Y	Y
Jurisdiction f.e.	Y	Y	Y	Y	Y	Y
1st stage Fstat					4.647	6.010
Hansen J					0.8012	0.5173
Observations	6,967	6,967	6,967	6,967	6,890	6,890

Note: N=6,967. The sample used in this analysis uses 658 unique municipalities that conducted reassessment at least every 15 years or more frequently. Endogenous variables instrumented: Years since last reassessment, cycle, interaction between dummy of each cycle and years since last reassessment, State aid for reassessment, Assessment ratio or level of assessment. (Added instrument: neighbors' mean frequency)

Table 9. Effect of reassessment lag on ETR: Virginia

	OLS	Control function			
DV: ETR	(1)	(2)	(3)	(4)	(5)
$since$	-0.027	0.021*	0.001	0.026*	0.001
	(0.022)	(0.012)	(0.036)	(0.015)	(0.036)
$since \times every2Y$	0.014			0.004	-0.021
	(0.023)			(0.006)	(0.043)
$since \times every3Y$	0.016			0.004	-0.025
	(0.026)			(0.007)	(0.045)
$since \times every4Y$	0.029			0.004	-0.007
	(0.022)			(0.007)	(0.043)
$since \times every5Y$	0.024			-0.002	-0.016
	(0.022)			(0.009)	(0.044)
$since \times every6Y$	0.023			0.002	-0.014
	(0.022)			(0.010)	(0.044)
Exogenous		$\widetilde{HD}, \widetilde{MV}$	$\widetilde{HD}, \widetilde{HPI}$	$\widetilde{HD}, \widetilde{MV}$	$\widetilde{HD}, \widetilde{HPI}$
Year f.e.	Y	Y	Y	Y	Y
Jurisdiction f.e.	Y	Y	Y	Y	Y
Observations	1,862	1,049	1,053	1,063	1,053
R-squared	0.906	0.914	0.913	0.914	0.913

Note: N=1,862 with a sample of 133 unique municipalities for fixed effect models. N=1,063 with 79 unique counties and independent cities for models using $\widetilde{HD}$ and $\widetilde{MV}$ as exogenous determinants of reassessment frequency in the control function models. N=1,049 with 78 unique counties and independent cities where $\widetilde{HPI}$ is used as a key exogenous determinant of reassessment frequency. Constant for model (1) is 5.2%.

Table 10. Event Study: NY jurisdictions that ever conducted annual reassessments

	3 consecutive years		4 consecutive years	
	Three residential sub-classes	Single family residential	Three residential sub-classes	Single family residential
DV: $-\ln(COD)$	(1)	(2)	(3)	(4)
D^{-5}	-0.018	0.024	0.128	0.095
	(0.106)	(0.094)	(0.227)	(0.244)
D^{-4}	-0.106	-0.091	0.224	0.102
	(0.101)	(0.072)	(0.186)	(0.189)
D^{-3}	0.036	0.099	0.054	0.041
	(0.117)	(0.102)	(0.161)	(0.162)
D^{-2}	0.000	0.047	0.074	0.027
	(0.142)	(0.111)	(0.182)	(0.183)
D^{0}	0.120	0.259	-0.315	-0.482
	(0.118)	(0.266)	(0.278)	(0.345)
D^{1}	-0.135	-0.051	0.101	0.228
	(0.250)	(0.270)	(0.315)	(0.357)
D^{2}	0.256	0.426***	0.273	0.273
	(0.162)	(0.150)	(0.390)	(0.375)
D^{3}	0.027	0.113	0.696**	0.674**
	(0.366)	(0.346)	(0.277)	(0.289)
D^{4}	-0.018	0.024	-0.081	-0.144
	(0.106)	(0.094)	(0.164)	(0.195)
D	0.068	0.034	-0.008	0.045
	(0.112)	(0.115)	(0.152)	(0.149)
Observations	445		427	

Note: Bootstrapped standard errors with 500 replications in parentheses. The treated units are assessing jurisdictions in NY that conducted annual reassessment for three to four consecutive years between 2003 and 2008, while the comparison group is consisted of NY assessing jurisdictions that did not started annual reassessment only after 2008. *** p<0.01, ** p<0.05, * p<0.1

Figures

Figure 1. Trend in reassessment and aid receipt: New York

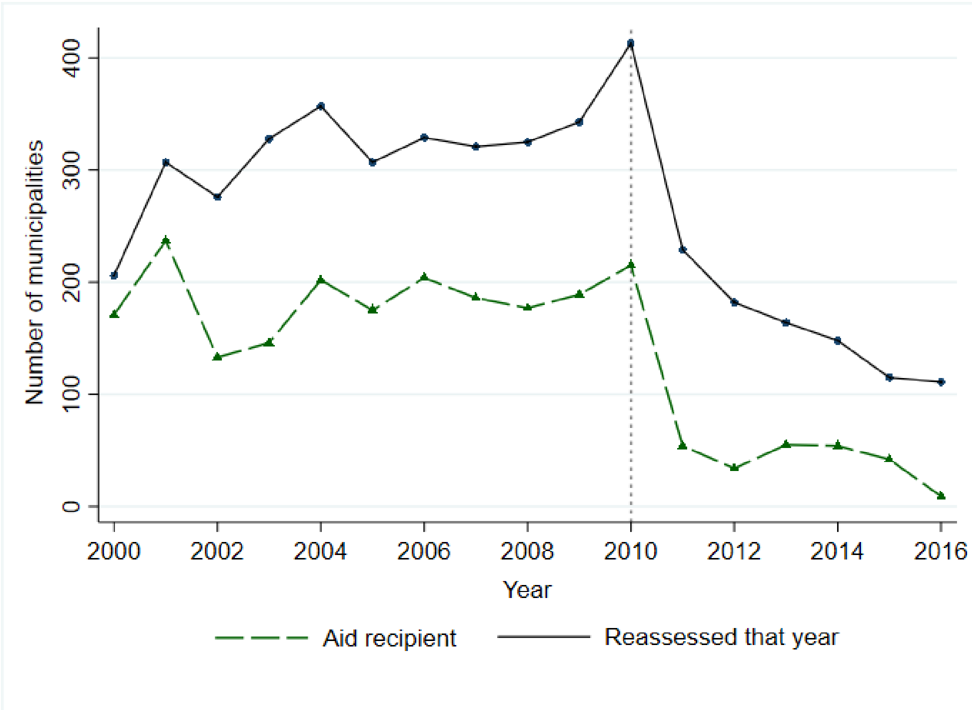


Figure 2. Stylized facts: Assessment uniformity and maximum reassessment cycle in Virginia

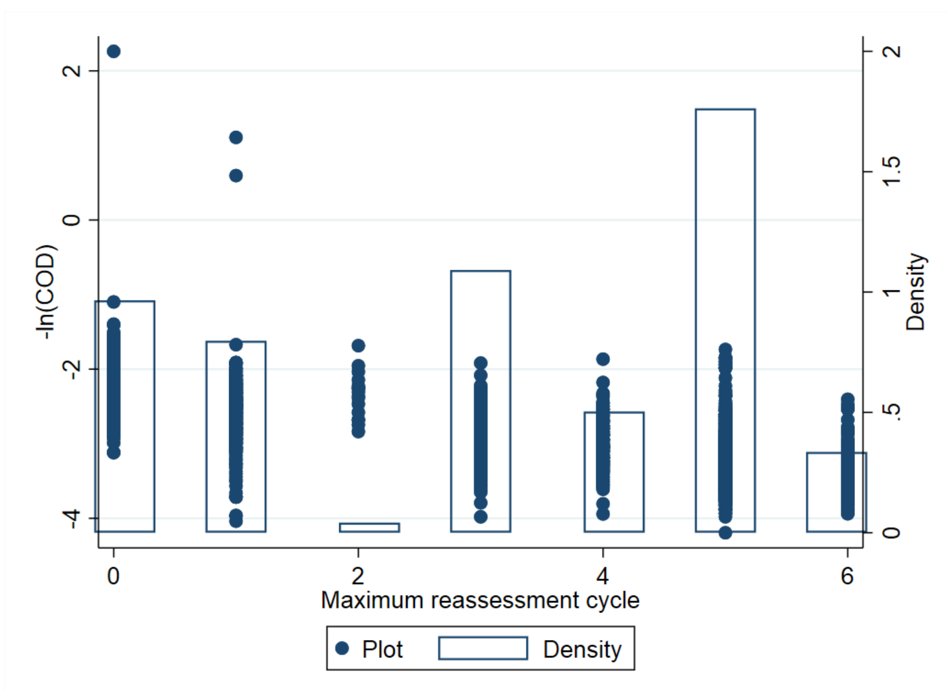
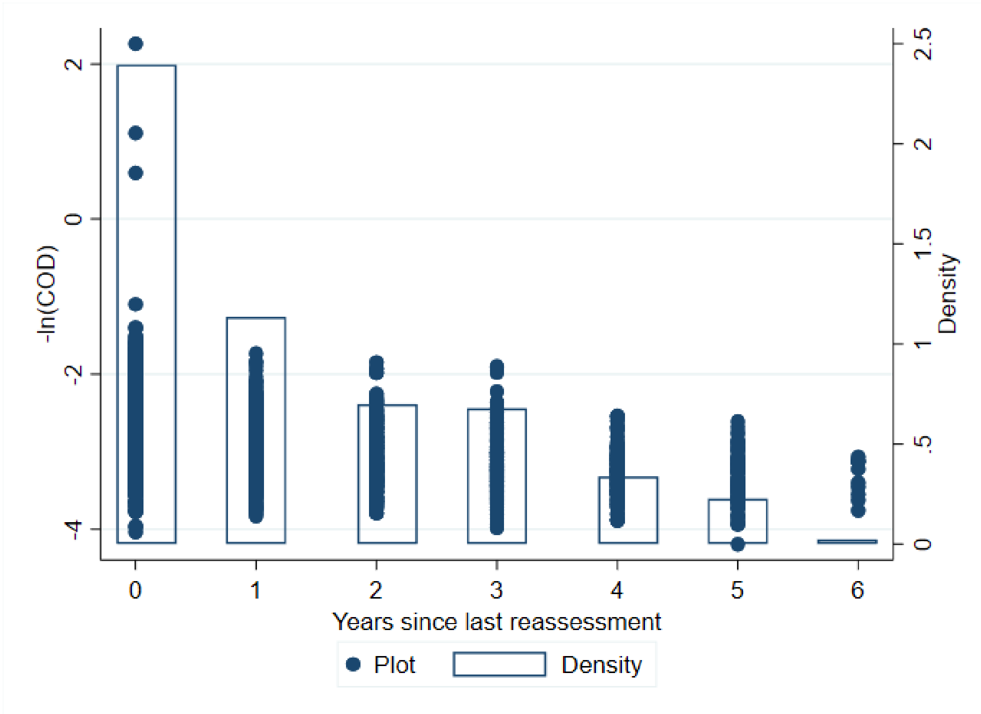
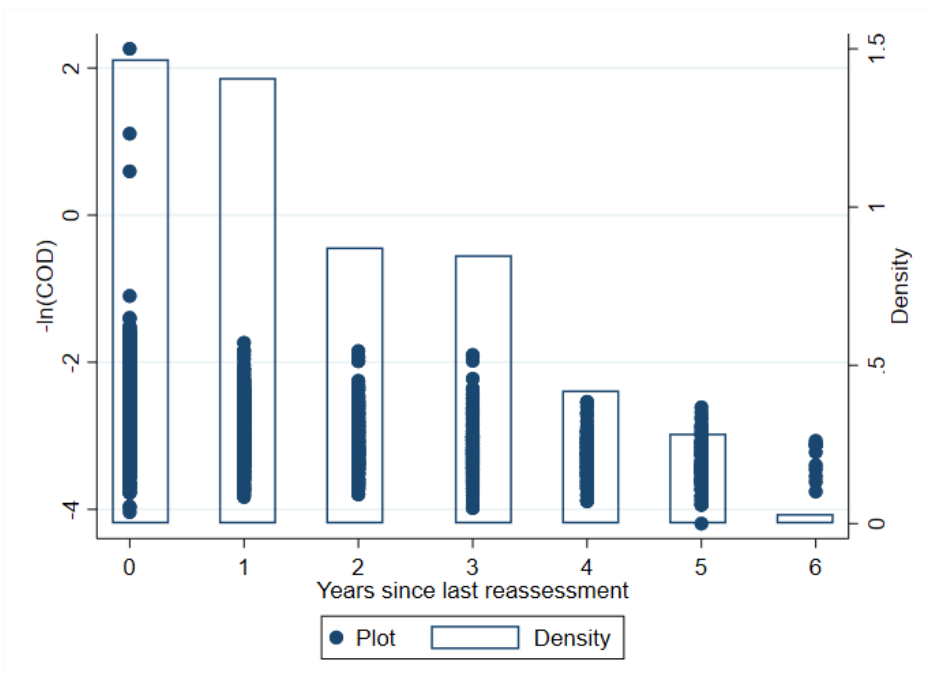


Figure 3. Stylized facts: Distribution of reassessment lag among tax assessing jurisdictions in Virginia



Panel A. All tax assessing jurisdictions



Panel B. Excluding jurisdictions conducting annual reassessment

Figure 4. Predictive outcome estimates across the distribution of reassessment lag, Virginia

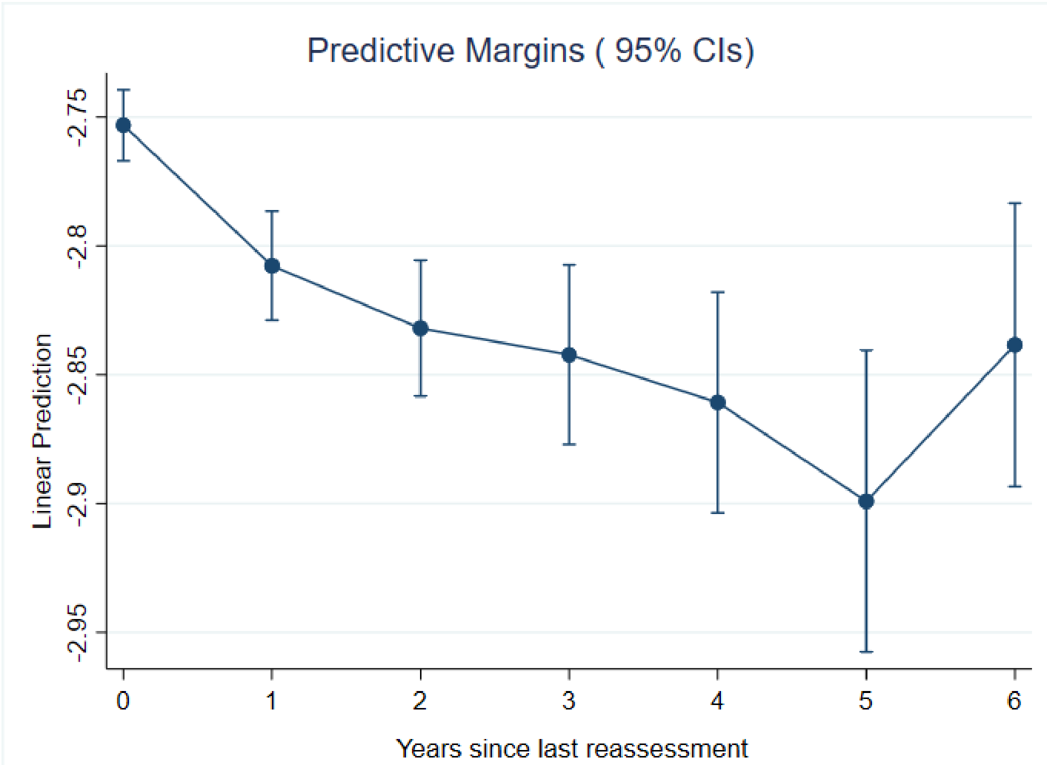
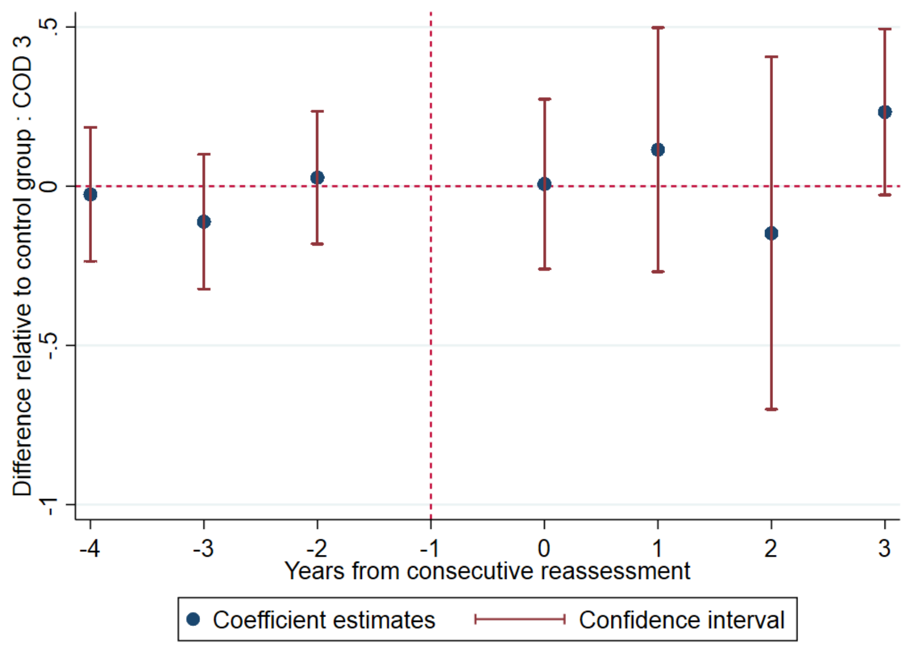
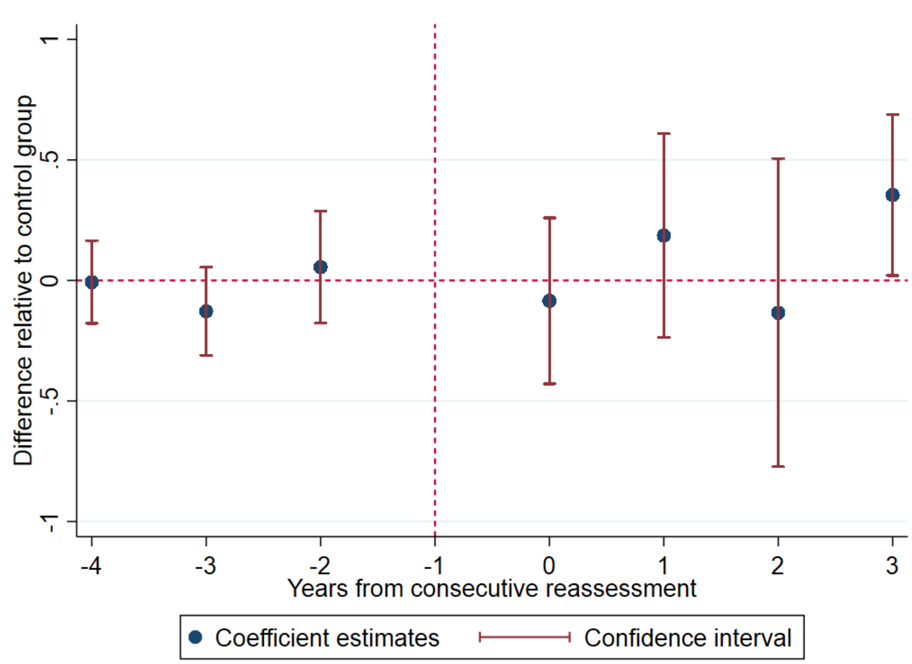


Figure 5. Event study estimates (Four consecutive years of reassessment)



Panel A. COD of three residential sub-classes



Panel B. COD of single family residential class only

Appendix

Table A1. Seven plus consecutive annual reassessment: No comparison group

Start Annual	(1) COD3	(2) COD1	(3) ETR	(4) Nominal tax rate
D^{-4}	-0.118	-0.228	0.760	7.794
	(0.162)	(0.206)	(2.058)	(15.625)
D^{-3}	-0.073	-0.141	2.995	10.793
	(0.202)	(0.215)	(2.694)	(15.724)
D^{-2}	-0.051	-0.051	0.136	2.570
	(0.168)	(0.163)	(2.096)	(1.638)
D^{0}	0.290**	0.185**	-0.428	0.458*
	(0.114)	(0.089)	(1.225)	(0.238)
D^{1}	0.210	0.075	2.187	0.550**
	(0.166)	(0.153)	(3.100)	(0.251)
D^{2}	0.392**	0.281	-1.995	0.590**
	(0.199)	(0.221)	(2.178)	(0.270)
D^{3}	0.417*	0.352*	-1.051	0.610**
	(0.224)	(0.185)	(1.588)	(0.293)
D^{4}	0.513**	0.427	-2.026	0.674*
	(0.238)	(0.272)	(2.646)	(0.398)
D^{5}	0.122	0.135	-1.380	0.432
	(0.458)	(0.407)	(2.094)	(0.274)
$Constant$	0.030	0.051	1.146	-3.564
	(0.088)	(0.087)	(1.695)	(2.412)
R-squared	0.517	0.522	0.239	0.901

Note: N=700, Bootstrapped standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table A2. Triennial reassessment (at least two cycles): No comparison group

Start Annual	(1) COD3	(2) COD1	(3) ETR	(4) Nominal tax rate
D^{-4}	-0.104	-0.095	-0.000	0.341**
	(0.067)	(0.087)	(0.001)	(0.167)
D^{-3}	-0.045	-0.029	0.001	0.118
	(0.146)	(0.116)	(0.001)	(0.231)
D^{-2}	-0.040	0.006	-0.001	-0.044
	(0.139)	(0.161)	(0.001)	(0.148)
D^{0}	0.010	0.001	0.001	0.100
	(0.048)	(0.055)	(0.001)	(0.121)
D^{1}	0.146	0.171*	0.002***	0.265**
	(0.153)	(0.104)	(0.000)	(0.107)
D^{2}	0.017	-0.020	0.001	0.040
	(0.157)	(0.137)	(0.001)	(0.122)
D^{3}	0.121	0.197**	0.002	-0.159
	(0.133)	(0.089)	(0.006)	(0.144)
D^{4}	-0.282***	-0.270***	-0.000	-0.273
	(0.030)	(0.053)	(0.004)	(0.166)
D^{5}	-0.043	-0.029	-0.003	-0.137
	(0.121)	(0.130)	(0.003)	(0.263)
D^{6}	-0.104	-0.095	-0.000	0.341**
	(0.067)	(0.087)	(0.001)	(0.167)
R-squared	0.402	0.406	0.927	0.950

Note: N=4,683 Bootstrapped standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

YUSUN KIM

426 Eggers Hall Syracuse University, Syracuse NY 13204
Phone: (315) · 708 · 5716 Email: ykim79@syr.edu

EDUCATION

Maxwell School, Syracuse University
Ph.D. Candidate in the Department of Public Administration (2019, expected)
Dissertation: *Three Essays on Property Tax Administration*
Committee Members: Drs.Yilin Hou (Advisor), John Yinger, Sarah Hamersma, Michah Rothbart, Michael Wasylenko

Seoul National University, South Korea
M.A. Public Administration, Highest Honors, 2013

Korea University, South Korea
B.A. Economics, 2011

RESEARCH INTERESTS

Public Financial Management, State and Local Finance, Property Tax
Health Policy, Public Health Insurance, Medicaid

PUBLICATIONS

Hamersma, S., Hou, Y., **Kim, Y.**, & Wolf, D. (2018). Business Cycles, Medicaid Generosity, and Birth Outcomes. *Population Research and Policy Review*, 1-21. https://doi.org/10.1007/s11113-018-9483-3

Kwon, H.J., Cook, S. & **Kim, Y.** (2015). Shaping the national social protection strategy in Cambodia: Global influence and National ownership. *Global Social Policy*, 15(2), 125-145.

RESEARCH IN PROGRESS

'How Reduction in Mandated Medicaid Spending Affects Local Fiscal Behaviors: Evidence from New York'

'Estimating Returns to Scale in Property Assessment: Coordination among Property Assessment Jurisdictions in New York'(with Yilin Hou & John Yinger)

'Impact of Property Assessment Frequency on Assessment Uniformity in New York and Virginia'(with Yilin Hou)

'How Do Local Governments Respond to State Aid Eligibility Requirement: Evidence from Tax Assessing Jurisdictions in New York'

'Medicaid Local Mandate and Local Fiscal Behaviors'(With Pengju Zhang)

'Impact of Budget Stabilization Fund on Public Employment'(with Yilin Hou)

CONFERENCE PRESENTATIONS

Yilin Hou & **Yusun Kim**, 'Reassessment Cycle and Equity in Property Tax' at the American Society for Public Administrtion, D.C., March, 2019.

Yusun Kim, ‘How Reduction in State Mandated Medicaid Spending Affect County Fiscal Behaviors: A Study of New York Counties’ at the Association for Budgeting and Financial Management Conference, D.C., October, 2018.

Yilin Hou & **Yusun Kim**, ‘State Aid, Regular Property Reassessment, and Assessment Outcomes: Evidence from New York State’ at the Association for Budgeting and Financial Management Conference, D.C., October, 2018.

Yusun Kim, ‘State Mandate Relief and Local Property Tax Cut: How Reduction in New York Mandated Medicaid Spending Impacts County Fiscal Behaviors’ at the Western Social Science Conference, San Antonio, April, 2018.

Sarah Hamersma, Yilin Hou,**Yusun Kim** & Doug Wolf, ‘Medicaid and Birth Outcomes over the Business Cycle’ at the Midwest Economic Association meetings, Evanston, March, 2016

Yilin Hou & **Yusun Kim**, ‘Tracing the Impact of Countercyclical Fiscal Policy on Employment’ at the Association for Public Policy Analysis and Management Conference, D.C., November, 2016.

Yilin Hou & **Yusun Kim**, ‘State Fiscal Reserves and Employment Stabilization: Empirical Tests’ at the Association for Budgeting and Financial Management Conference, D.C., October, 2015.

Huckju Kwon, **Yusun Kim** & Sarah Cook, ‘Shaping the National Social Protection Strategy in Cambodia: Global Influence and National Ownership of a Policy’ at the Social Policy Association, Seoul, July, 2014.

Lai, Ngan-yin Lai & **Yusun Kim**, ‘Extending Health Protection to the Informal Economy and the Poor: A Comparative Analysis of Thailand and Indonesia’, at the International Labor Office (ILO) Annual Conference on Regulating for Decent Work, Geneva, July, 2011

TEACHING EXPERIENCE

Teaching Assistant for MPA Health Economics and Policy Spring 2019

Teaching Assistant for MPA Quantitative Methods II Spring 2019
(Led review sessions for assignments and exams)

Teaching Assistant for MPA Quantitative Methods II Spring 2018
(Led exam review sessions for two sections)

Co-instructor for STATA Course, Six-week course Spring 2018
(Prepared course materials and taught the course for three weeks)

Teaching Assistant for Public Budgeting, Summer Session #2 Summer 2015-2017
(Led review sessions for assignments and exams)

Teaching Assistant for Economics for Policy Decision Making Fall 2016
(Lectured a session on public goods and led review sessions for exams, Evaluation 4.68/5)

Co-instructor for Excel Course, Six-week course Fall 2016
(Designed and taught the course for three weeks)

Teaching Assistant for Public Budgeting Fall 2016
(Lectured two sessions on cost analysis and led review sessions)

Instructor for a one-day Executive MPA Math Camp Fall 2015
(Prepared course materials and lectured as the main instructor)

OTHER WORK EXPERIENCE

Graduate Research Associate, Center for Policy Research, Syracuse University 2014-2019

Research Assistant, Economic Budget Analysis Division,
National Assembly Budget Office, Korea 2013-2014

RESEARCH AWARDS AND FELLOWSHIPS

Best Graduate Paper Honorable Mention Award, Western Social Science Association	2018
Summer Research Award, The Maxwell School, Syracuse University	2014 - 2018
Graduate Associate Award, The Maxwell School, Syracuse University	2014 - 2018
Department Travel Grant, Syracuse University	2014-2018
Highest Honors in Masters of Public Administration, Seoul National University	2013

LANGUAGES & TECHNICAL STRENGTHS

Languages	English(Fluent), Korean(Native), Mandarin(HSK6)
Data analysis and skills	ArcGIS, LaTeX, Stata, SAS, SPSS

REFERENCES

Yilin Hou
(Advisor)
Professor
Public Administration
Syracuse University
(315)443-9072
yihou@maxwell.syr.edu

Johnny Yinger
(Committee Member)
Trustee Professor
Economics & PA
Syracuse University
(315) 443-9062
jyinger@maxwell.syr.edu

Sarah Hamersma
(Committee Member)
Associate Professor
Public Administration
Syracuse University
(315) 443-5455
sehamers@maxwell.syr.edu